154

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EDITED BY
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WITH THE CO-OPERATION OF PROF. SIR F. C. BARTLETT AND PROF. C. D. BROAD

CONTENTS

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TABLE OF CONTENTS
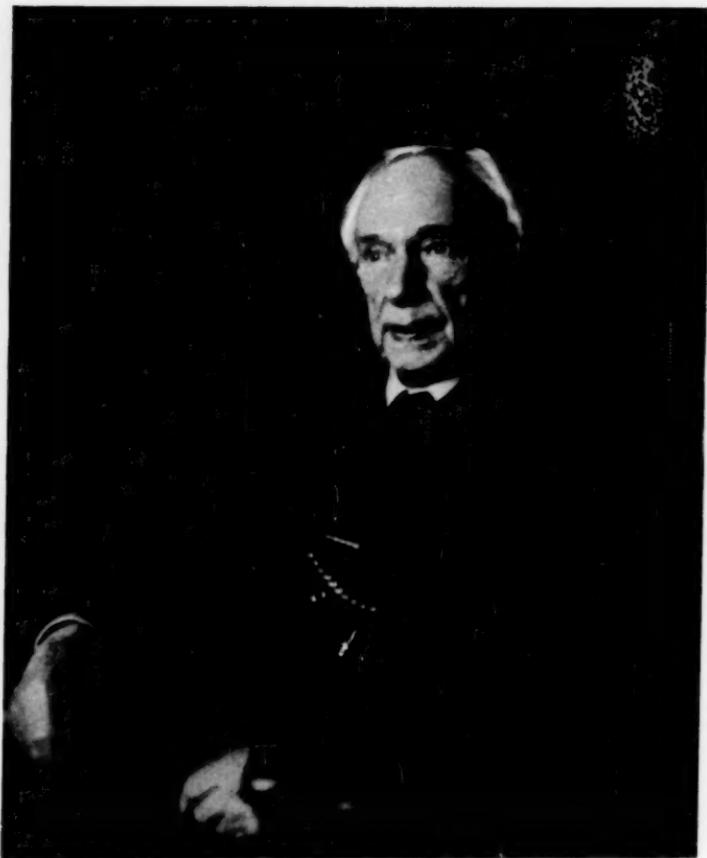
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MIND

A QUARTERLY REVIEW

OF

PSYCHOLOGY AND PHILOSOPHY

I.—MOTIVES AS EXPLANATIONS

By N. S. Sutherland

To give a motive for an action is one way of explaining it : if we are to understand how the word " motive " functions, we must understand how an explanation of an action in terms of a motive differs from other possible explanations of it.   As a preliminary it may be worth looking at certain types of action for which there can be no explanation in terms of motives.

One class of acts [1] for which motive explanations cannot be given is the class known as " reflex acts ".   It normally makes no sense to ask what motive a person has for jerking his leg when the patellar tendon is tapped or for blinking when something is waved in front of his eyes.   If we ask why someone jerked his leg or blinked in these circumstances, the answer that is given is a straightforward causal answer :   the tap was the cause of his leg jerking, the object waving in front of his eyes was the cause of his blinking.

A second class of acts for which there is no explanation in terms of motives is the class of acts performed out of habit.   To say that someone locked the back door purely from force of habit is not merely to give a different kind of explanation of his action from that given if we say " His motive for locking the back door was to keep burglars out ", it excludes the possibility that this was the reason why he locked the back door.   Of course when he first started locking the back door, his motive may well have been to keep burglars out, but once locking the back door has become a

[1] The words " act " and " action " will be used in what follows as they are appropriate :   the distinction between them is not discussed since it is of doubtful relevance to the question of motives.

habit it is no longer done from a motive. The test for whether someone is now locking the back door merely from force of habit or for a particular motive is whether he would tend to continue to lock the back door when the circumstances in which the original motive could operate are changed. Thus if there is a further locked door beyond the back door it may be unnecessary to go on locking the back door in order to keep burglars out, but he will tend to go on locking the back door under these circumstances if it was done from force of habit. The situation is complicated by the fact that if the circumstances have not changed, we can still say of someone who locks the door through force of habit, that he has a motive for locking the back door, namely to keep burglars out; one can have a motive for an action, perform the action and yet still not perform it from that motive. Thus it is possible to consider whether all of a list of suspects had a motive for committing a murder, although only one of them did in fact commit the murder. If we say of someone that he did something purely out of habit, this excludes the possibility that he did it from a certain motive—it does not exclude the possibility that he had a motive for doing it : even if he had a motive for doing it, the motive was not the reason why he did it if he did it merely out of force of habit. To say that someone had a motive for doing something he has done, is not necessarily to say that he did in fact do it from that motive ; and to say that someone did something purely from force of habit is to exclude the possibility that he did it from any motive. Of course there are occasions when we are not quite certain how far something is done from a motive and how far it is done through force of habit ; someone can lock the back door partly out of habit, partly because he wants to keep burglars out. The force of saying this would seem to be that he would have been less likely to lock the back door if he had not formed the habit of locking it, and he would have been less likely to lock the back door if locking the back door did not serve to keep burglars out. It remains true that if someone did something *purely* from force of habit, it excludes the possibility of a motive explanation so that actions performed purely from force of habit must be excluded from the class of actions for which there is a motive explanation.

A third class of action for which there can be, in ordinary language, no explanation in terms of a motive is that of unintentional actions, *i.e.* actions performed by accident or by mistake. Someone who gives the wrong change by mistake, does not give the wrong change for a certain motive, *e.g.* financial

gain—if he does give the wrong change for a motive, then he has no longer made a mistake.

A fourth class of actions for which there is no motive comprises actions which we perform merely for the sake of performing them. If someone suddenly starts to run, and is asked what his motive was, he may truthfully reply : " I had no motive : I just wanted to run " or " I just like running ". This may be the explanation of why hunger can be a motive for stealing but not for eating. To be hungry means to want to eat : thus to say of someone that he is eating because he is hungry involves little more than saying he is eating because he wants to. He is not eating in order to serve some further end.

A fifth class of actions for which no explanation in terms of motives can normally be given is that of actions which are the direct expression of a mood or an emotion. We normally have no motive for yawning—if we do have a motive this implies that we are not yawning merely because we are tired or bored. If someone yawns out of boredom, then his motive for yawning was not to encourage his guests to depart : he had no motive, he just yawned out of boredom. If on the other hand he yawned in order to encourage his guests to depart then he was not yawning out of boredom, though he may well have been bored at the time he yawned. To give the motive explanation of yawning is to exclude the explanation of yawning as the outcome of a mood. Again someone who throws things in anger, would not normally be said to be throwing things from a motive : he may be just throwing things out of anger. Of course he may be angry and throw things from a motive : if the things he throws are aimed at someone else, his motive for throwing them may be to hurt someone else, or to make them stop talking, but in this case he did not just throw the things out of anger.

These are only a few of the things we do for which no explanation in terms of motives can be given : there are others, some of which will be examined below. Examining the five different types of action, it would seem that they have one thing at least in common : they are not done in order to achieve some further end. The man who locks the back door from force of habit, or who runs for the sake of running, or who makes a mistake about the change may be attending to what he is doing and he may be doing it carefully, but he does not do it in order to achieve something else : he does it because he always does it (habit), because he enjoys doing it, or because he is tired or cannot count, but not in order to achieve some further end.

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place in order to secure some further end : this is not true of the knee jerk, but it might be true of the eye blink.   In fact if we were going to give an explanation of the eye blink from an evolutionary point of view, we might well say that it had come into existence in order to protect the eye from the impact of extraneous bodies.   There are several reasons why we prefer to give an explanation of the eye blink in causal terms rather than in terms of some end served by it.   One such reason is that it is comparatively unvarying : a body which is not moving directly towards the eye causes the eye to blink as readily as one that is ; we still blink even when we know that someone's fist is going to stop just short of our eye.   It is relatively easy to specify the external causes of the eye blink and the blink does not vary much with the probable consequences of whatever it is that causes it.   Moreover even when someone has a motive for not blinking, he is still likely to blink under certain circumstances.   Thus a person may try not to blink in order that someone else may remove a fly from his eye, but still be unable to help blinking. In this case the blink no longer serves the purpose of protecting the eye from the extraneous bodies but it still occurs. This is a similar test to that which was applied above in considering how to decide when an action was done from habit rather than from a motive, and the results of such a test in the case of the eye blink explain why we prefer to give a causal explanation of it rather than explain it by the end it serves since it occurs on a particular occasion irrespective of whether or not it is likely to achieve that end on that occasion.

It would appear then that at least some actions which cannot be explained in terms of a motive are actions which are not done to achieve some further end.   We must now ask whether to assign a motive to an action is merely to explain that action in terms of some further end to be achieved by the action.   It is necessary to insist on the word " further " : as already shown, something done for its own sake is something done without a motive.   A murder which was committed merely because the murderer wanted to kill a particular person and for no other reason would be a crime without a motive : if he killed because he was jealous of the murdered man or because he wanted his money, then the murder would have a motive and the crime would be correspondingly easier to solve.

The great majority of motive explanations of actions do fit the paradigm of explaining the action by assigning a further end towards the accomplishment of which the action was directed. If one asks what someone's motive was for doing X, an answer of

the kind " He did X because he wanted Y ", or " He did X in order to achieve Y " is surely the most common. We do sometimes, however, answer by giving a single word which describes some motive which is of common occurrence in human beings. In *The Concept of Mind*, Ryle concentrates in his account of motives mainly on the use of such single words (*e.g.* " vanity ", " patriotism ") to answer questions about motives. We must now ask how far explaining behaviour by the use of one of these words is explaining it by pointing to the end to which it is directed. In many cases this still seems to be an accurate description : thus to say that someone's motive for doing something was ambition is to say that he did it *in order to* obtain a more important position or something of this kind. Again, to say that someone acted from patriotic motives is to say that he did what he did in order to help his country.

Other words of this type are less straightforward—what end does a vain person pursue when he does something from a motive of vanity ? Part of what we mean by being vain is desiring the admiration of others, usually for something trivial : if a woman buys a new hat in order to be admired by others, her motive for buying it may be said to be vanity. This does not exhaust the meaning of the word " vanity " : another part of what is meant by being vain is that a person enjoys contemplating his own imagined good points, or that he dislikes contemplating his own failings. Actions performed in order to give one a chance to admire oneself might be said to be performed from a motive of vanity. Vanity can be a woman's motive for looking in mirrors, or—later in life—for not looking in mirrors. Thus the ends which a vain person pursues are varied and cannot be described in a single phrase. Moreover, although " vanity " may function as a motive word it often does not : a person may be vain, know he is vain and deliberately refrain from doing things out of vanity. If he looks pleased when praised for something trivial, or downcast when criticised, we shall know he is vain : but vanity is not his motive for feeling pleased or downcast. We do not talk of having motives for being pleased or downcast : being pleased or downcast do not serve any further ends nor are they actions. There is a clear contrast here with a word like " patriotism " : we would not say of anyone that he was genuinely patriotic unless he did things or in certain circumstances would do things in order to help his country ; we can say of a person that he is vain even though he does not pursue any ends connected with vanity. In order to call someone vain, it only has to be true that he is unduly pleased to receive praise or

admiration for something trivial and that he is unduly sensitive
to criticism. Usually, however, a vain person will do things to
secure admiration and avoid criticism and it is in just this case
where he is pursuing the sort of end that a vain person pursues
that we can use the word " vanity " as a motive word. Words
which describe character traits may themselves call attention to
a greater or lesser extent to the ends which a person pursues :
words like " patriotic ", " avaricious ", " vindictive " cannot be
truthfully applied to a person unless he consistently does do or
would do things in order to help his country, in order to accumu-
late money, or in order to revenge himself on others. The
word " vain " is much less directly connected with any end.
Other character-describing words are not connected in any way
with the ends a person follows. " Conceit " is an example of one
such word. A conceited man is simply a man who estimates
his own abilities as much greater than they are : there is no end
which a conceited man typically pursues. In consequence, it is
never possible to say " His motive for doing that was conceit ".
Of course a man may do something " out of conceit " or " as a
result of conceit " : all this means is that if he had not over-
estimated his own abilities he would not have done whatever he
did. " He applied for the post out of conceit "—if he had not
overestimated his own worth he would not have applied, since he
would have known he could not succeed. Conceit was not a
motive for his applying for the post, because the word " conceit "
does not call attention to the end he was pursuing when he applied
for the post.

It is worth examining some more of these character-describing
words in order to show that whether or not the nouns formed
from them can be used to give the motive of an action depends
upon the extent to which the character-describing word itself
classifies men according to the ends which they pursue. A
cowardly man is a man who seeks to avoid any dangerous situ-
ation : we call him cowardly, in virtue of the fact that some of
his actions are directed towards a certain end namely the avoid-
ance of personal danger. Since cowardice is intimately connected
with an end, it is appropriate to use the word " cowardice " to
assign a motive : for example, " His motive for avoiding military
service was cowardice ". A timid man on the other hand does
not characteristically pursue any end : a man is timid if he
flinches in the face of danger, if his knees tremble when the shells
start falling, if he cowers in front of the school bully. He does
not necessarily seek to avoid danger, although he reacts in a
characteristic way when exposed to it. Because of the lack of

connection between "timidity" and the ends which a man pursues, we cannot say "His motive for avoiding military service was timidity". Similarly, "bravery" cannot be used as a motive word : a brave man is one who remains steadfast in the face of danger—he does not necessarily seek out dangerous situations. On the other hand "bravado" involves the idea of doing things in order to appear brave, or doing things in such a way as to appear brave. It is thus possible to use the word "bravado" to assign a motive. Thus we can say "His motive for climbing the cliff was bravado", but not "His motive for climbing the cliff was bravery". It is not intended to suggest that the extent to which character-describing words are used to classify men according to the ends which they pursue is an all or none affair. The application of different character-describing words depends to a greater or lesser extent upon considering a man's actions according to the ends which they serve. Words like "intelligence", "stupidity", "vivacity", "sarcasm", "scepticism", "bravery", "conceit", "timidity" have very little connection with the ends a man pursues; words like "patriotism", "vindictiveness", "greediness", "avarice", are intimately connected with ends ; and words like "vanity", "cowardice", "generosity", are somewhere in between in so far as the ends of a man's actions are relevant to considering whether or not these words can be truthfully applied to him.

Before leaving character-describing words, it is worth observing that they may be logically more complex than anything we have yet hinted at. As an example of such a word let us consider the word "jealousy". This word can certainly function in motive explanations, and in fact it occurs frequently to specify a motive in detective fiction and only slightly less frequently in criminal proceedings. Here the end of the action to which the word "jealousy" points is very much harder to specify than the ends to which words like "ambition" and "patriotism" point. However, it is possible to give some description of the ends which a jealous man pursues. He may try to prevent whoever are the objects of his jealousy from obtaining something he covets himself, he may try to harm them in some way, or to make others dislike them (e.g. by passing disparaging comments): the notion of wishing to harm someone in some way or colloquially wishing to take someone down a peg seems central to the use of "jealousy" to describe a motive. The situation, however, is much more complicated than this: not only does "jealousy" call attention to a particular kind of end which dominates a person's behaviour, it also calls attention to a specific reason why that end should

occupy him. There are several other motives which may lead someone to harm others, *e.g.* revenge, cannibalism, sadism, etc. Moreover, someone may do harm to others without a motive, yet his action may still be explainable. Thus harm may be done out of tactlessness, yet tactlessness is not a motive since it does not point to any end served by the action it is introduced to explain. Jealousy indicates not merely some wish to harm or take down a peg, but the reason for that wish as well : to be jealous is to wish someone ill, or to wish that someone has not got something *because* he has something you have not and would like to have. Thus where jealousy is used as a motive it refers to some sort of purposive behaviour, but the use of the word does more than merely indicate the purpose of the behaviour, it indicates the reason why one has that purpose. Thus even a word such as " jealousy " which functions in a very complex way seems to be used to classify behaviour according to the ends to which it is directed when it functions as a motive word.

It should be noted that even the character-describing words which do classify men in accordance with the ends to which their behaviour is directed do not always function as motive words. We have already shown that being pleased or downcast cannot be explained in terms of motives. Yet a man may be made miserable in certain circumstances through ambition : explaining someone's misery in this way is just to say that he is an ambitious man and that ambitious men are made miserable by the sort of circumstance in which he is. Being miserable does not serve any further end. In addition there are certain actions which may be explained by pointing to some character trait connected with the ends a man pursues without giving a motive explanation of the action. Thus ambition may drive a man to commit suicide : it is not a motive for suicide, since nobody commits suicide in order to achieve the ends which are characteristically pursued by an ambitious man, unless of course they commit suicide in order to attain a posthumous notoriety. His motive for committing suicide may be to end the misery attendant on thwarted ambition, but not directly to achieve the ends pursued by an ambitious man. Similarly a patriot may beat his breast because the country is going to the dogs : he beats his breast because he is patriotic, but patriotism is not a motive for beating his breast. Even phrases which often function as motive explanations can explain an action without functioning as a motive explanation. Thus if we say " Little Willie is crying because he wants to go to the circus ", this is not giving a motive for his crying since the

crying does not serve any further end. Of course it will count as a motive explanation if Willie is crying in order to persuade someone to take him to the circus : we can then say " His motive for crying is that he wants to be taken to the circus ". The crying is now being put on in order to achieve some further end, which the motive explanation calls attention to.

At this point it is necessary to put in a caveat. There is something unusual about the form of words " Willie's motive for crying was that he wanted to go to the circus ". Indeed we seldom use the word "motive" in everyday language. It is more usual to ask not " What was X's motive for doing that ? " but " Why did X do that ? ", and more usual to reply not " His motive was ..." but " He did it because ...". The word "motive" is seldom used outside the courtroom, the psychologist's consulting room, the novel about character, and the philosophical article. The reason for this may be that the very precision of the word makes it pedantic to use it in most situations in which we are asking for an explanation of behaviour. Thus if we ask what someone's motives are for doing something, by the way we have phrased the question we have already excluded explanations in terms of habit, doing it because he wanted to do it, doing it out of boredom, and doing it by accident. Moreover, in replying to a " Why ? " question about behaviour what is said makes it clear what type of explanation is being given without the use of the word " motive ". If we say " He did X because he wanted Y " then it is clear that a motive type explanation is being given and there is no necessity to say " His motive for doing X was that he wanted Y ". We are normally interested only in the explanation of the behaviour, not in the type of explanation, and since the word " motive " calls attention to the type of explanation its use is often otiose. It is in situations where we wish to attain greater precision in our explanations that the word tends to be used. This is not to say that its use in ordinary language would be mistaken : it would merely be pedantic. We are interested here in the logic of motive-type explanations of behaviour and therefore we shall continue to talk of " explanations in terms of motive " to refer to this type of explanation even though the word " motive " often does not appear in the sentences giving this type of explanation. Similarly the word " cause " is often not used in giving causal explanations, e.g. " What made the engine seize up ? " " It seized up through lack of oil."

We are now in a position to understand why to ask a person's motives is very often a step towards discrediting his actions. To ask for someone's motives for being polite is to suggest that he is

not habitually polite, or that he is not polite merely because he wants to be polite, but that the politeness is serving some further end. It is perhaps partly for this reason that the words " goodness " and " badness " do not function in motive explanations of behaviour. A good man does good things just because he wants to and not to achieve some further end. Although of course the good man presumably pursues good ends, he also follows certain rules of conduct and if he is to be thought of as good he must be following those rules because he wants to and not in order to achieve some further end. In addition the class of good ends would be an extremely wide class—it does not delimit sharply a class of ends towards which actions can be directed in the same way that a word like " patriotism " or " ambition " does.

I have so far attempted to show that to assign a motive for an action is to explain the action in terms of the end towards which it is directed. I have attempted to support this thesis by showing that this explains why certain words and phrases can function in motive explanations of actions and why others cannot, and it explains why some actions can be explained in terms of motives and why others cannot. The question of a motive can only arise when there is an action to be explained : there is no such thing as simply having a motive—we only have a motive for doing something. We can have a motive for doing things which we do not in fact do : this merely means that if we were to do something or to have done something, it would have tended to produce some further consequence which we want. This case may be treated as derivative from the case where an action has actually been performed and we explain it in terms of a motive : when we ask for a motive explanation of an action not in fact performed, we ask whether the person in question does in fact pursue ends which such an action might lead to. It is for this reason that we can say someone *has* a motive X for an unperformed action Y rather than that if Y had been performed, his motive for performing Y would have been X. The hypothetical action is explained in terms of the ends a man pursues—if the man actually does pursue these ends then the motive is not hypothetical, it actually operates in this person and might have led him to perform the hypothetical action even though it did not. Similarly we can give reasons why an accident might have occurred at a particular bend even though the accident did not occur and the reasons are not hypothetical ones : we can say " the reason an accident might have occurred at this bend is that the road was very slippery ". We cannot use a similar construction with the word " cause " because if the effect is hypothetical the cause

must be also : if something is not followed by an effect, then it cannot be a cause of that effect—it cannot have existed or the effect would have occurred.

I now wish to pass to an attempt to elucidate further what is meant by explaining actions in terms of motives, and I shall concentrate on the case where the actions are actions which really have occurred. In *The Concept of Mind* Ryle is tempted to assimilate motive explanations to dispositional explanations, and the reason for this may be his preoccupation with motive explanations made by giving a character trait. Many character traits are in fact purely dispositional and the way in which a word like "timid" functions may not be so very different from the way in which a word like "fragile" functions. I have argued, however, that words for character traits which are purely dispositional in this way cannot feature in motive explanations ; it is only where such words categorise behaviour in relation to the ends the behaviour serves that such words can be given as motive explanations. It is true that Ryle subsequently warns us of the distinction between motives and habits and writes " In ascribing a specific motive to a person we are describing the sorts of things that he tends to try to do or bring about ", but the assimilation of motive explanations to dispositional explanations may be more misleading than helpful. If to give a motive explanation is to explain an action by referring to the end it serves then there is a big difference between a motive explanation and a dispositional explanation of the sort " the glass broke because it was fragile ".

In order to bring out this difference it may be helpful to consider under what circumstances an explanation in terms of ends is useful. Consider someone searching for a mislaid letter : he may feel in his pockets, rummage through his drawers, scan any envelopes he discovers, while ignoring other bits of paper, and the process is likely to continue until finally he discovers the letter in question. It would be extremely hard to give any systematic account of this behaviour without any reference to the letter the finding of which is in some sense the end towards which his behaviour is directed. Thus behaviour of this kind, rummaging, scanning letters. etc., is likely to continue until he has found the letter in question : without knowing what the letter is, it would be impossible to forecast either what forms his behaviour will take or when it will stop. Without talking about the letter, it is not possible to explain why he looks in just the places he does, why he seldom looks in the same place twice, why he attends to letters and not to books.

When we seek to explain anything we always make use of

similarities between different events or series of events. To categorise series of events in terms of whether or not they tend to arrive at the same outcome is one sort of similarity between series of events. Two men try to get to Piccadilly Circus : one starts from Hampstead, goes to a tube station, and takes a southbound train. Another starts from Brixton, enters his car, and drives north. We explain the dissimilar behaviour of these two men by saying in both cases that they wanted to get to Piccadilly Circus. Part of what is entailed by this is that had their starting points been reversed, they would, other things being equal, have adopted opposite courses of action. We can certainly make predictions based on statements about motives and there is no particular difficulty about verifying the statements. Like ordinary causal statements they imply unfulfilled hypotheticals. One test for whether a particular action occurred for a certain motive is to prevent that action having the consequences which are envisaged as the end of the action and to discover whether other actions which might be expected to achieve those consequences will occur. It is precisely in virtue of the extent to which categorising actions in terms of the ends towards which they are directed will give us predictive power that this is a useful method of categorising and explaining actions. The fact that someone is ambitious may be the most important kind of similarity between his behaviour today and yesterday—important for categorising his behaviour systematically and for making predictions about his behaviour tomorrow. Of course in order to make predictions about what someone will do, it is not enough to know the ends which he pursues or is pursuing, we must also know what he knows about the circumstances he is in, and how far he can predict the consequences of his own actions.

If this account of what it is to assign a motive to an action is correct, then motives are in themselves neither dispositional explanations nor causal explanations. Motive explanations could not be given if there were not regular causal sequences in the world since actions could not be directed to ends unless one action was more likely to be followed by a certain consequence than another. Moreover, it is not intended to suggest that behaviour usually explained in terms of motives may not have a causal explanation. A consideration of the circumstances under which in everyday life explanations in terms of ends are preferred to explanations in terms of causes will throw further light on motive type explanations.

There are many phenomena in the realm of inanimate objects which exhibit something like purposive behaviour, and yet we

are rarely tempted to describe them as being purposive or to explain them in terms of ends rather than causes. For example, water tends to find its own level, yet the behaviour of water is not normally thought to be purposive. There seem to be at least two major differences between the behaviour of water and the behaviour of systems for which we do prefer an explanation in terms of ends. (1) The final state towards which water tends, *i.e.* being at its own level, does not vary either from one piece of water to another or from one piece of water at one time to the same piece of water at a different time. (2) The extent to which the behaviour of water will vary according to the circumstances in which it is placed is extremely limited. It flows downwards until either it reaches its own level or until it reaches some substance impermeable to water. If it is flowing towards a basin which will trap it above its own level, it will not do anything which would result in its avoiding the basin, nor if it is trapped in a basin will it do anything which would result in its escaping from the basin and reaching lower levels. It is possible to state simple laws governing the behaviour of water from which, knowing its present position, we can predict accurately its future position without referring in the laws to any sort of end pursued by the water and this is possible precisely because of the two features of its behaviour already pointed out namely, that its behaviour does not change systematically in such a way as to achieve different ends at different times nor does its behaviour alter systematically in such a way that it will achieve the same end under a very wide variety of initial circumstances. This is a further illustration of what was said above about motives, that an explanation of the behaviour of a system in terms of ends is useful to the extent to which that system varies its behaviour systematically in such a way as to tend to bring about the same result irrespective of the initial conditions in which it is.

It is of course possible to build machines which exhibit behaviour which we are much more tempted to call purposive than the behaviour of water. The essential feature of such machines is that the causal linkages inside the machine are such that its output will vary concomitantly with its input in such a way that the same end result is obtained irrespective of considerable variations in input. To be thought of as purposive the end result must be such that it would not have been arrived at without any output being given by the machine despite differences in the environments, *i.e.* we are tempted to call the machine's activities purposive if they tend to produce the same end result when taken in conjunction with variations in the machine's environment, but

neither the different environments nor the different outputs of
the machine at different times would by themselves have led to
the same end. Similarly the connections in the human brain
might be of the same kind, except that the variety of initial con-
ditions under which behaviour will vary in such a way as to tend to
produce the same result is extremely great and there are a great
many different end results towards which behaviour of the same
person may tend at different times. In these circumstances a
knowledge of the ends towards which a given person's behaviour
is directed together with a knowledge of what he knows about the
outside world and what his predictive capacities are is the simp-
lest way of predicting what he will do. In addition it is usually
the results of a person's behaviour which we are interested in
rather than the exact form of the behaviour, so that even actions
are categorised in everyday speech largely by the immediate end
that they secure rather than by actual physical similarities between
actions, e.g. in terms of muscle contractions etc. : thus " posting
a letter " categorises an action in terms of the immediate end the
action secures. For everyday purposes it is unnecessary to concern
ourselves with the different ways in which this action might be
performed—e.g. with the left hand, the right hand, grasping the
letter between the thumb and forefinger, or between the fore-
finger and middlefinger, etc. In giving a causal account of posting
a letter, however, these things would need to be taken into
account—the causal account would explain why at a certain time
exactly such and such muscle contractions occurred : without
thinking in terms of ends we could not disregard the details of the
way each action is performed, and the explanation would be
correspondingly very much more complex, and correspondingly
less useful for everyday purposes. If this is correct, it explains
why explanations in terms of motives are confined at present to
the explanation of human behaviour.

It must be remembered that the fact that explanations of
behaviour are couched in everyday speech in words which are
not used in the explanation of the behaviour of animals or
inanimate objects does not necessarily reflect a fundamental
difference between men and the rest of creation : it merely reflects
a difference in our way of thinking about them. It does not
preclude the possibility of giving causal explanations of human
behaviour nor does it preclude the possibility of giving explan-
ations of inanimate systems in terms of ends, if it were found
convenient to do so. Thus the fact that words like " motive "
are rarely used in explanation of animal behaviour despite the
obvious convenience of such explanations for certain actions of

certain animals, may point not to any actual difference between ourselves and animals which makes it appropriate to give this type of explanation in the one case and not in the other, but to a reflection in language of theological views about the difference between men and animals.

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II.—THE ORDINARY LANGUAGE TREE

By Fred Sommers

An important part of any investigation into the meaning of an expression E consists of finding what may be called its sense location. This is done by noting which expressions may be conjoined with E and which may not. " E is that expression which goes well with A, C, or G in a sentence but E fails to make sense when used with B, D, F or H, etc." When the mutual sense relations of A, B, C, D, E, F, G . . . are known, then we have a map in which each expression has a sense location with respect to the other expressions under consideration. The question we shall consider is whether the natural language provides any rules for the construction of such a map, whether there is, as it were, an invariant structure to " linguistic cartography " in terms of which it would be possible to give the sense location of any of the expressions in the language. To this question we shall eventually offer an affirmative answer.

The theory of meaning adopted here is a current one. It is the theory of meaning-in-use. Employing a convenient distinction of Ryle's between two kinds of knowing, we may say that a knowledge of meaning is a " knowing how " rather than a " knowing that " : to know the meaning of an expression is to know how to use it. Such knowledge includes an ability to formulate a piece of non-absurd discourse containing the expression. Thus to know the meaning of a word is to know how to formulate some sentences containing the word, to know the meaning of a sentence is to know how to formulate some coherent discourse containing the sentence. It is almost true to say that the meaning *is* this use, *i.e.* the meaning of E (if " E " is a word or phrase) is the set of sentences containing E and that my knowledge of the meaning of E grows (though not in direct proportion) with my ability to formulate more and more sentences in which E has a proper use. A complete knowledge of E would then be represented by the set of all such sentences. The trouble with this view is that even such a set would not specify uniquely the meaning of any one expression since the set would also specify the meaning of all those expressions which have the same use— at this level of use. For example, the word " short " might be specified by the sentences in which it has a non-absurd occurrence from a purely semantic point of view, but those sentences may also specify the word " tall ".

160

We must therefore keep in mind that a map of sense relations giving the locations of a group of expressions does not tell the whole story of " their use in the language ", *i.e.*, their meanings. Nevertheless, we shall see that such a map removes ambiguity, ensuring univocity for the expressions located on it. For this reason we shall identify the sense of an expression with its location on a map. This entails a distinction between *sense* and *meaning*, a distinction which we shall enforce rather than justify. The *sense* of an expression will be its location with respect to other expressions, its semantic range. It is what it " makes sense " with as contrasted with what it fails to make sense with. Its *meaning* is governed only in part by sense rules. " Tall " and " short " may have the same " sense "; it is because of other rules governing their use that they diverge in meaning. Thus, giving the sense of an expression is not yet the same as giving its meaning. One who wishes to know more about the meaning of a given located expression will enquire at that address.

The notion of sense location and sense relation is, however, still too vague to be of much service. Some time in specifying it more closely will not be misspent.[1]

We shall restrict attention to subject-predicate sentences. In what follows " an expression " will be a word or phrase which can occur as the subject or the predicate of a well-formed subject-predicate sentence. Two expressions will be said to have the sense relation " U " if they can be used together in a subject-predicate sentence. Examples of expression-pairs which are U-related are (husband, rational), (tall, tree), (sensation, itch).

[1] To know the meaning of E is to know how to formulate utterances which contain E and which satisfy quite a number of rules of different sorts. Such rules have a certain order of priority, *i.e.* you can neither violate nor satisfy a rule R_{n+1} unless a rule R_n has been satisfied. For example, the word " I " must be used in an utterance which satisfies formation rules (R_1), before it can occur in a sentence which violates type or " sense " rules. It must satisfy these sense rules (R_2) before it can violate consistency rules (R_3). It must satisfy R_3 before it can fail to satisfy rules governing pragmatic appropriateness, for example, empirical truth. Knowing the meaning of " I " is knowing how to satisfy all these rules in some utterances containing " I ". But at each level of use it is possible to formulate utterances which satisfy the rules up to that level.

" I am a prime number " is correct with respect to R_1.

" I am prime minister and someone else is too " is correct with respect to R_2.

" I am prime minister " is correct with respect to R_3.

" I am concerned mainly with sense rules in this paper " is a correct use of " I " with respect to R_4.

11

If two expressions, E and F, are U-related then the expression-pair (E, F) will be said to have the *sense value* " U " and we shall indicate this by " U(E, F) ". The notion of sense values is a convenient one since for our purposes expression-pairs can be treated as sentences and the analogy to truth values is helpful.

An expression-pair will be said to have the sense value " N " if its component expressions cannot be used together in a sentence. Examples of such pairs are N(anger, man), N(mood, itch), N(angry, valid). Such pairs cannot be used together in a subject-predicate sentence. Sentences formulated with expressions which are N-related are " nonsense " even though they may be well-formed.

In sentences which have more than two expressions we shall consider two cases. Either two predicates are predicated of one subject. In that case if the sentence makes sense we consider the two predicates to be U-related. For example, U(angry, sad) since there is a perfectly good sentence in which both " sad " and " angry " are predicable of the same thing, *e.g.* a husband. On the other hand N(angry, valid) since there is no sentence in which these expressions are predicated of the same thing.

Sentences in which one predicate is predicated of several subjects are another matter. Here we do not say that the two subjects are U-related even though such sentences make good sense. Thus N(anger, man) despite the fact that there are good sentences in which these expressions can serve as subjects together. (For example, ' Man and Anger can both be discussed '.) The fact that a predicate like " can be talked about " makes good sense when used with expressions cannot establish those expressions as U-related.

Thus we have the following interpretation of the U-relation. If (SP) makes sense then U(SP). If (SPP') makes sense then U(PP'). If (SS'P) makes sense then it does not follow that U(SS').

Another way of expressing this makes use of the common forms for symbolic translation. Let " Ax Bx " stand for a sentential function such as ' x is A or x is B ', ' If x is A then x is B ', ' x is B and x is A ', etc. Let Q stand for one of the quantifiers such as " $\sim \exists (x)$ ", " (x) ", etc. Thus " Q(Ax Bx) " is a sentence ; whether this sentence makes sense depends solely upon what A and B are. For example if A = author, B = book, Q(Ax Bx) is not significant, while if B = bold, it is significant. A change in the quantifier (say from " $\sim \exists (x)$ " to " (x) ") or a change in the logical connective (say, from " If . . . then " to " and ") has no effect on the significance or non-significance of Q(Ax Bx). If we indicate that a sentence is significant by placing brackets about it, the U-relation may be defined thus :

$$[Q(Ax\ Bx)] \equiv U(AB) \equiv U(BA) \text{ df.}$$
$$N(AB) \equiv \sim U(AB) \text{ df.}$$

Any sentence translatable into the form Q(A B) may now be said to have the same sense value as its expression-pair (AB). We may therefore speak of the sense value of sentences as well as of expression-pairs. More simply we may treat any expression-pair as if it were a sentence suitably quantified and connected. Those who like to deal solely in terms of ordinary linguistic forms can with suitable adjustment translate expression-pairs as familiar sentences with two expressions. For more than two expressions we have not defined a sense relation. However, subject-predicate sentences of more than two expressions are reducible to two or more simple sentences containing only two expressions.

We note that a significant sentence SPP' while not directly translatable into Q(Px P'x) does nevertheless ensure the *significance* of the latter sentence. For this reason we say that SPP' implies U(PP'). On the other hand the significant sentence SS'P does not ensure the significance of Q(Sx S'x) though it does yield [Q(Sx Px)] and [Q(S'x Px)] and hence tells us that U(SP) and U(S'P).

Finally, U(AB) = U(BA) and N(AB) = N(BA) express the important fact that the well-formed convers. of every significant sentence is significant, of every non-significant sentence, non-significant. Of course, this holds true of other operations such as obversions, etc., as well.

This fact permits us to ignore the order of expressions in a pair and to consider only their mutual use or misuse together. (It will be seen later that expressions on a map of sense relations lie on directed paths. But such an ordering of expressions is definable solely in terms of sense relations between expression-pairs not initially ordered.)

We see that a pair of expressions gets a sense value from the fact that certain sentences do or do not exist in the language. Thus (anger, triangularity) is N in sense value because there is no significant sentence in which either " anger " or " triangularity " is the subject while the other is the predicate, nor is there a sentence in which both are predicated of the same subject.

The existence of non-absurd sentences is a fact. Those who are ready to agree to this platitude often balk at the other fact : the existence of absurd sentences. For when we say that (anger, triangularity) is N in value or that any sentence which may be formed with these expressions as subject and predicate is N in value we say this because we do not find a non-absurd sentence

such that U(anger, triangularity).   But this is no different from
saying that the sentence " Anger is triangularity " is known to be
nonsense.   And here those who delight in setting up contexts for
a metaphorical use of these expressions and others like them can
always protest against this evaluation of the sentence.   One may
here counter that the recognition that this can only be done by
giving an expression a " metaphorical " use is an admission that
on the " ordinary use " the sentence is not only false but it makes
no sense.   Also it is quite clear that the theory of meaning-in-use
is based on the premise that there is a known body of nonsense to
avoid, as " misuse ".   I would like to elaborate on this a little and
take the opportunity to extend the use of the symbolism a bit as
well.

To have any meaning at all an expression must have some use
with other expressions, *i.e.* it must occur in some U-sentences.
We may call the set of sentences in which E has unchallenged use
the " U-set of E ".   We shall use the symbol " UE " to stand
for this set of sentences.   Similarly " NE " will be used to stand
for the set of well-formed sentences in which E is clearly misused.
If I know the meaning of E then I can formulate some sentences
in the U set of E.   I can also recognize some misuses of E should
these occur.   (In practice this is mainly the avoidance of sen-
tences in which E would be misused.)   There is something
seriously wrong with my knowledge of the word " anger " if I
could not recognize its misuse in the sentence, " His anger was
five feet from the lamp ".   If I could not recognize *any* misuse of
" anger ", I could not be said to know its meaning at all.   My
command over an expression must include the abilities both of
formulation and of avoidance if indeed these are " two " abilities
in any important sense.   We may express this fact by saying that
for any given knowledge of a language, the U and N sets of the
known expressions in the language are not empty.   This does not
mean that if I know the meaning of the word " anger " that I must
know the sense value of every expression-pair or every sentence
containing this word.   I may, for example, be puzzled by its
occurrence in (3), " His anger was completely unconscious ", and
wonder whether this embodies a use or misuse of " anger ".
Here I might wish to call upon someone (perhaps one who is an
expert in " doing philosophy ") to help me come to a decision on
the sense value of this sentence.   I would still know the meaning
of " anger " since its U and N sets are not empty.   But knowledge
of meaning, being a species of " know-how " admits of degrees and
I would wish to know more of the meaning by adding to my
abilities of formulation and avoidance.

It will be convenient to assign yet another symbol to stand for those well-formed sentences, which for any given knowledge of a language are of doubtful sense value. Thus (3) might belong to this doubtful set for my knowledge of " anger ". We shall use " DE " to stand for the set of sentences containing E which for my knowledge of the meaning of E cannot be assigned either to the set UE or NE. The task of analysis here is seen as the sense evaluation of some of the sentences in DE. The U, N, and D sets for any expression may vary from person to person. What does not vary is the fact that any knowledge of an expression is constituted (in part) by the sentences in its U and N sets.

The sentences in the U and N sets of an expression are the data which is used for a sense evaluation of the sentences in its D set. We therefore will call such sentences (i.e. the U and N sentences) " data " sentences. When I am puzzled about the sense value attaching to a particular occurrence of an expression then I use the data sentences for that expression to evaluate its occurrence in the puzzling sentence. When it is a matter of persuading someone else, I will make an appeal to ordinary use (data sentences whose sense values are held in common by a number of people engaged in the task of evaluating a D-sentence). How we proceed from the values given in the data sentences to assign a value to a doubtful sentence is a question which we will consider more formally later. We shall see that the very possibility of such sense evaluation depends upon the fact that the data sentences can be mapped in terms of a formal structure of sense relations obtaining among the expressions of the language. We shall also see that once the data sentences are chosen the task of sense decision is simple. It is not a matter of persuading and bringing evidence which is never conclusive. The " persuasive " element in " philosophical analysis " is due not to the *method* of arriving at a decision from the data ; it is due to the fact that there is often some disagreement about the data, especially the data sentences in the N-sets.

We have so far given some formal expression to certain facts about the natural language. We noted that its expressions may be used as indices to group the sentences of the language into sets. Thus if A, B, C, D, E, . . . J, K are expressions belonging to a language which I understand, there will be sets UA, UB, UC . . . NA, NB, NC . . . and, most probably, DA, DB, DC, etc. In this way we may represent our knowledge of the meaning of these expressions, *i.e.* our command, so far as it extends, of their use and misuse. In this way also we have a rough organization of the mutual sense relations obtaining among the expressions. This is

so because the sense values of our data sentences are the same
as the sense values of the expression-pairs which occur in them.
But even aside from the fact that many expression-pairs have no
value assigned to them owing to the deficiency of our knowledge
of the language, this sort of organization is hardly a structure.
The sentences in UA may not constitute even a partial meaning
of A since they may embody several different uses of A, resulting
in several locations on a map.   These uses are not distinguished
by a simple collection of all the sentences in which A has use.
For example, in the U-set of " rational " we will have sentences
(man, rational), (argument, rational), and so on.   We normally
distinguish between senses, that is, *uses*, of ' rational '.   In order
to do this we implicitly employ the concept of " a use " as a
criterion of univocity.   There is, in other words, an ideal point at
which we stop making distinctions between one use and another.
If there were not, we should be driven to the doctrine that each
sentence in which a given expression occurs constitutes a new and
slightly different use of that expression.   I shall not pause to
refute this view ; its deficiencies are obvious.   But if there is
some limit to use distinction short of individual occurrence it
becomes incumbent on us to state this limit as clearly as we can.
In terms of our symbolism, the question, " What counts as a
use ? " is a demand for a criterion for grouping the U-sentences of
E into proper subsets in each of which E has one and the same
use.

Where shall we look for the rules which govern the subsets ?
It is quite clear that on the theory of meaning-in-use, a criterion
of " use " is another way of talking about a criterion for univocal
meaning.   Univocity is, however, an ideal of any procedure which
distinguishes different meanings of an expression.   The sensible
approach, therefore, is to examine that familiar piece of linguistic
behaviour called " making a distinction ".   Also any technique
which is employed for making distinctions is equally a technique
for evaluating a D-sentence.   Suppose given a set of data sen-
tences, some of which contain the expressions " A " and " B " I
apply some rule which renders a decision on a sentence D(AB).
Let us say that this decision is that N(AB).   Any such decision is,
however, qualified by the evidence.   We say, " In *this* sense of
A, *i.e.* as embodied in these data sentences, A is misused with B in
the sentence (AB) ".   This leaves open the possibility of giving
A another sense and formulating sentences in which A has a use
that is different from the one it has in the data sentences which
were brought as evidence for the invalidity of (AB).

By thus formulating sentences in which A occurs, perhaps, as

a consistent metaphor, we can always reinstate a sentence on which a sense decision "nonsense" has been reached. This, however, is not always a voluntary procedure. More often a distinction in the use of A is forced upon us. We find a set S_1 of sentences whose sense values are given in ordinary language and to these we apply a rule R which gives us N(AB). We find another set S_2 and again apply R and this time we get U(AB). We then say that the use of A in S_1 is different from its use in S_2 and that (AB) is N or U depending on which sense of A is embodied in (AB). Also, if (AB) is a data U-sentence to begin with, the existence of S_1 and S_2 will place U(AB) in S_2 and the same distinction will obtain.

All sense decision is the application of the discovery of a particular use of A to the sentence D(AB). If now we examine the procedures of logicians who claim to render sense decisions on (AB), we shall find what for them constitutes *a* use of an expression. The rules of sense decision are rules which implicitly define "use". For this reason, a look at some current procedures for evaluating D-sentences promises to repay us with the answer to the question, "What counts as a use ?"

The following two examples illustrate a basic procedure employed in decisions of sense value for doubtful sentences. The first is typical of Ryle :

> Words like 'distress', 'distaste', 'grief', and 'annoyance' are names of moods. But 'hurt', 'qualm', 'itch', when used literally, are not names of moods. We locate hurts and itches where we locate the grit and straw that we feel or fancy we feel. Yet 'hurt' and 'itch' are not nouns of perception either. Hurts and itches cannot be distinct or indistinct, clear or unclear. CM244.

The second is typical of Wittgenstein :

> "Understanding a word": a state. But a *mental* state ?—Depression, excitement, pain, are called mental states. Carry out a grammatical investigation as follows: we say
> "He was depressed the whole day."
> "He has been in continuous pain since yesterday."
> We also say
> "Since yesterday I have understood this word."
> "Continuously" though ? PI, I, 59 n.

What Ryle and Wittgenstein are in effect doing is deciding upon the sense values of certain sentences which are (presumably) doubtful in sense value. Among these doubtful sentences are "An itch is a mood" (Ryle) and "Understanding a word is a mental state" (Wittgenstein). Their procedure is to examine other sentences whose sense value is held to be known and in this

way cast light upon the sense value of the doubtful sentence.
Thus Ryle finds two sentences :

   (1) ' An itch is locatable on the body '—a U-sentence.
   (2) ' A mood is locatable on the body '—an N-sentence.

He considers this to be evidence that ' An itch is a mood ' is N in
sense value. Incidentally, by adding the qualification that
' itch ' be used literally, Ryle is merely assuring us that his
argument only holds for that use of ' itch ' which is embodied in
(1). On some other (metaphorical) use of ' itch ' he does not
deny that ' An itch is a mood ' may make sense.

Similarly, Wittgenstein in testing his doubtful sentence finds a
pair of sentences each of which contains an expression of the
sentence he is testing. And here too, these known sentences
being opposite in sense value indicate that the doubtful sentence
is N. The known sentences are :

   (3) My mental state continued all day—a U-sentence.
   (4) My understanding of a word continued all day—an
       N-sentence.

In Ryle's example, let $A = $ ' itch ', $B = $ ' mood ', $C = $ ' locat-
able on the body '. Using brackets to indicate that a sentence
makes sense, we are given [AC] and $\sim$ [BC]. Ryle considers
this evidence against the significance of " AB ". His argument
in general is that if it makes sense to say that A is B (or to ask
whether A is B) then all other questions which may significantly
be asked of A may also be significantly asked of B. Since in our
case it makes sense to ask where an itch is located on the body
while it does not make sense to ask this about a mood, we have no
choice but to consider " An itch is a mood " to be a category
mistake. There are three equivalent ways of expressing this
argument :

$$([AC]\,\overline{[BC]}) \supset \overline{[AB]}\,;\; [AB] \supset ([AC] \supset [BC])\,;\; [AB]\,[AC] \supset [BC]$$

In Wittgenstein's argument, let $B = $ ' understanding a word ',
$A = $ ' mental state ', $C = $ ' continued all day '. If, Wittgenstein
argues, we say [BA], a rule is violated : If a sentence " BA "
makes sense, then whatever may be said or asked about A may
also be said or asked about B. But here it makes sense to ask
whether A is C while it makes no sense to ask whether B is C.
We therefore have no choice but to consider " BA " to be absurd.
Wittgenstein's argument is similar to Ryle's :

$$[AC]\,\overline{[BC]} \supset \overline{[BA]}\,;\; [BA] \supset ([AC] \supset [BC])\,;\; [BA]\,[AC] \supset [BC]$$

Taking the third formulations for both Ryle and Wittgenstein,
we have : (1) [AB] · [AC] $\supset$ [BC] and (2) [BA] [AC] $\supset$ [BC]. We

notice that (1)—Ryle's argument—has an antecedent which is in
the third figure of a traditional syllogism. (2) has an antecedent
which is either in the first or the fourth figure depending on the
order in which they are considered. Since $[BC] \vee [CB] \supset U(BC)$,
we can write (1) and (2) as follows :

$$[AB] \cdot [AC] \supset U(BC) \qquad 1.1$$
$$[BA] \cdot [AC] \supset U(BC) \qquad 2.1$$
$$[AC] \cdot [BA] \supset U(BC) \qquad 2.2$$

It would be tempting to generalize these forms and to say that (3)
$U(AB) \ U(AC) \supset U(BC)$. This would mean that a " sense argu-
ment " is valid in any of the four figures. (A " sense argument "
is an argument in which the significance of the conclusion follows
from the significance of the premises. It is not concerned with
the truth but with the sense of the premises and conclusion).

It is, however, easy to find many examples which show that the
second figure is not a valid sense argument form. For example,
let $A =$ ' new ', $B =$ ' bench ', $C =$ ' theorem '. Then it does
not follow from [BA] and [CA] that U(BC). But this means that
(3) is incorrect since it may be stated as $([AB] \vee [BA]) \ ([AC]$
$\vee [CA]) \supset U(BC)$. The validity of the first, third, and fourth
figures is expressed in this formula. Unfortunately it also con-
tains the second figure which we have seen to be an invalid
form of sense inference.

Rules 1.1, 2.1, 2.2 can be seen as expressing a semantic law to
the effect that any syllogism of the first, third, or fourth figure has
a significant conclusion if its " premisses " are significant. A
violation of this law in the data sentences (the ordinary use of
the language) is ruled out by the simple expedient of defining as
" equivocal " one of the three terms which appears in a triad of
sentences which violate one of these rules. This procedure is
familiar to us as " detecting " a fourth term. Apart from a rule
for its detection, there is no meaning to equivocation. The valid
sense figures 1.1, 2.1, 2.2, may therefore be construed as semantic
conditions for univocity. A " violation " can occur only where
there is no univocity. For example, we " detect " equivocation
in ' reasonable ' *because* of the rule 2.1 and the occurrence of
sentences like :

Nothing reasonable is female
Some of the speaker's remarks were reasonable
Some of the speaker's remarks were not female.

Those who claim that the equivocation is " obvious " will,
when asked to prove it, invariably bring forth other sentences
containing ' reasonable ' which violate one of these rules. For
example he would say, " Surely it is easy enough to show that the

word ' reasonable ' has more than one meaning in ordinary use. We say that something is reasonable in the sense that it behaves rationally or we say that it is reasonable in the sense of being cogent and true." In this argument for a distinction in the use of ' reasonable ', what is being done is the same as what was done earlier. We have groups of three sentences :

[Something reasonable is something behaving rationally]

[Something reasonable is something cogent and true]

~ [Something cogent and true is something behaving rationally]

The third sentence is nonsense and consequently there is an " obvious " equivocation in the middle term. Here a violation of the rule 1.1 is " obviously " not to be countenanced.

In short, if a term T is equivocal, whether this be obvious or not, it is a term which occurs as the middle term in ordinary use sentences whose " major " and " minor " terms have no use with one another. This holds, so far as we have seen for sentences which are in the first, third, or fourth figures. We shall in a moment show that this formulation requires serious qualification. But what will remain is that linguistic ambiguity is not a *fact* which can be obvious to anyone. It is a rule, or rather the apparent violation of a rule, which renders a term equivocal. A change in such a rule would mean a change in the very meaning of " equivocal " and we should no longer find a word equivocal in the way we did formerly.

We just saw that a sense argument in the " second figure " is not valid. Thus, there are many sentences in the language which have the form [BA], [CA], N(BC). It is this fact which invalidates the general formula : $U(AB) \cdot U(AC) \supset U(BC)$. The invalid second figure is contained in that formula. On the other hand it seems correct to assume that if a sentence " SP " makes sense then so does its converse " PS ". This leads us at once to suspect that the three so-called valid figures are not valid at all. The reason is that it is always possible to convert one or both of the premises of a valid figure and in this way to obtain an invalid second figure with no change in the significance of any of the sentences. Similarly, we can take three sentences already in the second figure in which the two " premises " are significant while the conclusion is absurd and by conversion in the premises we can obtain a first, third, or fourth figure, which violates one of the rules.

Nor is such violation a logical indication of equivocation. For example, let B = ' baseball game ', A = ' last a whole winter ', C = ' toothaches '. For convenience, let our three sentences form a truth-valid syllogism.

No baseball games last a whole winter.
Some toothaches last a whole winter.
Some toothaches are not baseball games.

These sentences are in the second figure. The premises are significant while the conclusion is nonsense. There is, however, no detectable equivocation since, admittedly, the second figure is an invalid sense rule.

But this same syllogism can be expressed in the first figure.

Nothing which lasts a whole winter is a baseball game.
Some toothaches last a whole winter.
Some toothaches are not baseball games.

In order to save rule 2.1 above (derived from Wittgenstein's argument), we should have to insist that some term here is equivocal. But this course is not open to us since nothing is different here than it was in the second figure where admittedly we have no warrant for a judgment of equivocation.

By converting both premises in an invalid second figure, we can develop the same argument against Ryle's third figure rule (1.1). Does this mean that the sense rules which are implicitly employed by both Ryle and Wittgenstein are unconditionally wrong ? If they were, it would surely be hard to account for their persuasiveness. For example, using Wittgenstein's first figure sense rule we were immediately led to see the equivocation in the word " reasonable " in a figure of the same mood as the one in which such reasoning has just been invalidated.

It is my contention that the arguments of Ryle and Wittgenstein are conditionally correct in a way which they have failed to make explicit. The condition under which it is correct to say that we cannot have two significant premises and an absurd conclusion must be one which holds for *all* of the four figures. Unless the condition is of this nature we can always reduce the so-called valid figures to the second figure which is not valid. Thus what we need is a linguistic condition under which even the second figure will be valid. It must be a condition which generally obtains for the first, third and fourth figures but rarely obtains for the second. Thus, it is not the figure which is important (except as a rough indication of the presence of this condition), it is the presence of a condition under which it is always true that $U(AB) U(AC) \supset U(BC)$.

The invalidity of the second figure (and subsequently of the other three as well) was clearly established by the occurrence in ordinary language of such data triads as [bench, new], [theorem, new], [bench, theorem] ; [game, lasts all winter], [toothache, lasts all winter], [game, toothache] ; [book, interesting], [author,

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interesting], [book, author]; etc. Predicates like "new", "interesting," "exists" have use with other predicates that have no mutual use. They are often called "high order" predicates. We may define as "higher" than B or C, a predicate A, which appears as the expression common to the U-pairs in a triad of expression-pairs which have the values of U, U, and N. Thus A is higher than B and C in the triad : U(AB) U(AC) N(BC). Also the middle term in any syllogism whose conclusion is nonsense but whose premisses are significant is, on this definition, higher than the major and minor terms. Being higher than another expression means no more than occurring as the expression A in such triads of data sentences as :

[AB] [AC] $\sim$ [BC]: [BA] [CA] $\sim$ [BC] : [AB] [CA] $\sim$ [CB], etc. In other words, $(A > B) \cdot (A > C) = U(AB) U(AC) N(BC)$ df.

We saw above that sometimes the high term is equivocal and other times it is not. For example, "reasonable" is higher than "female" and "remarks", and "circular" is higher than "apple" and "argument". In both of these cases we consider the high term equivocal in its use with the lower terms. On the other hand, "lasts a whole winter" is higher than "toothache" and "baseball game", "interesting" is higher than "book" and "author" but we have no grounds for considering these terms equivocal. Indeed we have as yet no grounds for considering any term equivocal. A good rule will enable us to pronounce "reasonable" and "circular" equivocal while not doing the same for "interesting" and "lasts a whole winter", etc. This would be a rule which has intuition on its side because it is the rule which our intuition implicitly employs. We have already said, it would be a rule which allows for Ryle's and Wittgenstein's arguments.

Thus a good rule will be one which (a) supplies a linguistic condition (preferably a simple one) whose presence will always allow us to say UU $\supset$ U for three expressions A, B, C, (b) enables us to distinguish between high terms which are univocally used with the lower terms and those which are equivocally used with them, (c) supplies us with a formal principle for the construction of a language map in which each expression has a unique sense location representing its use and misuse with other expressions.

" A is higher than B and C " has been defined as the occurrence of A, B, C in a triad : U(AB)U(AC)N(BC).

We shall now make the following assumption about ordinary language : If A occurs as higher than B and C then there is no expression D such that either B or C occurs as higher than A and D. Thus, if U(AB)U(AC)N(BC), then there is no D such that

either U(DB)N(AD) or U(CD)N(AD). For suppose there were a
D such that U(DB)N(AD). Then we have U(AB)U(AC)N(BC),
in which A is higher than B, and U(AB)U(BD)U(AD) in which B
is higher than A. Similarly, if U(CD)N(AD) we again have two
imcompatible triads : U(AB)U(AC)N(BC)—in which A is higher
than C—and U(AC)U(CD)N(AD)—in which C is higher than A.

Thus for any four expressions A, B, C, D, we may state a general
rule prohibiting inconsistent triads of the form UUN. The
occurrence of four expressions in sentences whose sense values are
such as to violate this rule is *logical* evidence for the equivocation
of one of these four terms. (*Strictly* speaking the logical evidence
we get is for a *fifth* term, though the short form of ordinary sense
arguments appears to argue from three terms to a fourth.)

The rule may be stated symbolically

$$\sim [U(AB)U(BD)N(AD)U(AB)U(AC)N(BC)] \qquad (1)$$
$$\sim [U(AC)U(CD)N(AD)U(AB)U(AC)N(BC)] \qquad (2)$$

from (1) we get U(BD)N(AD) $\supset \sim$ [U(AB)U(AC)N(BC)] from (2)
we get U(CD)N(AD) $\supset \sim$ [U(AB)U(AC)N(BC)]
Combining these we have the rule

$$N(AD)[U(BD) \lor U(CD)] \supset \sim [U(AB)U(AC)N(BC)] \qquad (3)$$

We shall henceforth refer to (3) as " R(U) ". This indicates
that (3) is a function of the sense value " U "—since " N " is
also a function of " U ", being its negation—but it also suggests
that (3) is a rule of use, and if I am correct, a fundamental one
implicitly defining what counts as " a use " at the semantic level
of linguistic use.

The expression U(AB)U(AC)N(BC) which appears as the
consequent of R(U) is equivalent to U(AB)U(AC) $\supset$ U(BC).
Thus R(U) states that the existence of sentences N(AD)[U(BD)
$\lor$ U(CD) is the condition for applying the ordinary sense rule
UU $\supset$ U for three expressions, A, B, C.

Consider again Ryle's argument against U(itch, mood). He
brings U(itch, locatable) from which would follow the (absurd)
conclusion that U(mood, locatable). According to the strict
rule the argument depends upon the fact that " itch " is known
not to be higher than mood and locatable. Thus the argument in
both cases depends upon the existence of sentences N(itch, X)
(U(mood, X) $\lor$ U(loc., X)). Since there are many such sentences,
e.g. let X = glumness, facial feature, serenity, blush, etc., the
second figure argument above is also valid.[1]

[1] Another familiar argument by Ryle is that against U(Seeing,
process). The *reductio* is U(seeing, process) U(process, takes time) imply
U(seeing, takes time). Here too the validity of the argument depends
upon the existence of sentences N(X, Process) U(Seeing, X) or N(X,

Both Ryle and Wittgenstein, to my knowledge at least, have been careful to use the short argument form only in cases where the antecedent condition obtains. And generally, all of us make use of the rule UU ⊃ U in *any* " figure " whenever that condition is present. It almost always is present in the case of the first, third, and fourth figures, since we normally do not use a locution like " Nothing that lasts a whole winter is a baseball game ". In the second figure the high term is the predicate, and what is normally the term which has a greater semantic range than the subject. Normally, in the first, fourth, and third figures, the middle term is *not* the high term of the three. This can easily be established by the search for a term D such that N(AD) [U(BD ∨ U(CD)].

R(U) is therefore nothing but the complete expression of the rule used to evaluate doubtful sentences or to prove the equivocation of some term ; unlike the rule UU ⊃ U, R(U) admits of no exceptions. As said earlier, any such rule, implicitly defines what counts as *a* use. The further importance of R(U) lies in the fact that it expresses the basic structural principle for a map of the sense relations between the expressions of the natural language. Before finally turning our attention to that topic I wish to make clear in what sense I consider the rule R(U) to admit of no exceptions. In the next few paragraphs I shall perhaps repeat some of the things which have already been belaboured. Unfortunately, the attitude that the natural language cannot be made to submit to rules, that ambiguity is a deplorable or exciting fact which ought to be accepted or celebrated, is so glibly accepted that a position which challenges this attitude is forced into some shrillness and repetition.

The theory of meaning-in-use is the theory that the meaning of a term is given by its use. It is also the theory that the use of a term is governed by rules. If we know the meaning of a term we can project beyond what we know; to be able to evaluate we must at least know part of its use. Generally this means the occurrence of the term in a strange context and a decision whether this occurrence constitutes a correct or incorrect use of the term. This ability implies the existence of a standard which enables us to determine what counts as a use in general. Ryle and Wittgenstein in their arguments are employing such a

Process) U(X, takes time). Ryle offers X = achievement as meeting this condition. Whether one agrees with the *data* values in such arguments is a separate question from the question of what, given a set of data values which we do agree are to be the premisses of our sense arguments, is the *proper form* of a sense argument. Our answer is : R(U).

standard. But it goes without saying that they did not invent it. We could find these rules implicitly used by anyone who is taking care to speak clearly and to use his terms without equivocation. It must, therefore, be possible to elicit standard forms of sense argument and to elaborate a theory of sense values and sense inference in much the same way as has been done for truth. The rule R(U) merely formalizes some ordinary sense arguments and states the full condition for their validity. It has on its side the fact that these arguments are in common use whenever we wish to make a distinction or to evaluate the occurrence of a term in an unfamiliar context. Also it has in its favour the fact that whenever we *refrain* from saying UU ⊃ U—the essential form of ordinary sense arguments for three expressions—we do so because the condition implicitly assumed for the validity of this argument is missing. For example, we refrained from using this argument in the syllogism given earlier :

(AB) [lasts a whole winter, ball game]
(CA) [toothache, lasts a whole winter]
(CB) ∼ [toothache, ball game]

Though the first two sentences (or expression-pairs) make sense, the fact that the third is absurd does not trouble anyone. Nor are we moved to consider any term here equivocal. There is in fact no expression D such that N(AD) [U(BD) ∨ U(CD)]. The same is true for other triads whose sense value for three expressions taken in pairs is UUN. We do not consider this to be evidence for equivocation in the " middle " term unless there is the antecedent condition given in R(U).

What would a violation of R(U) mean? If R(U) represents the form of ordinary sense arguments, any violation by a group of data sentences can only mean that some term is equivocal. Thus the rule expresses the formal semantic condition for univocity in the use of the predicates of the natural language. Since there can be no linguistic evidence against the rule—all counter-evidence being ruled out as equivocal by the rule—the rule itself is inviolable. Though no evidence against the rule can get a hearing, the evidence for it is simple : we use this rule to evaluate sentences of doubtful sense value and we use it to make distinctions of meaning. Those who wish to propose another rule which does the same are of course free to do so. This rule, I submit, is the one which is now in use. It defines what we now mean by " a use ". So far as I can see it is the simplest alternative to the unrestricted use of the rule UU ⊃ U.[1] That rule by itself is

[1] It can also be shown that the unrestricted rule UU ⊃ U is inconsistent with the view that there is misuse in any language.

too counterintuitive to be countenanced as a hypothesis. No rule is either correct or incorrect ; it is only in use or not. R(U) is.

I come back now to the question raised at the beginning of this paper. The rule of use enables us to get a graphic representation of the sense relations among expressions which are not equivocal, *i.e.* a map in which every expression is given a distinct location so that, for example, " reasonable " will have at least two locations—and to that extent—two senses as used with " female " and " argument ". Graphically, the question ' what counts as a use ? ' or ' what counts as a univocal meaning of an expression ' now becomes the question, ' what counts as a distinct location on a language map ? '

If we represented a U-relatedness between two expressions by a solid line connecting them and an N-relatedness by a dotted line then the sense values U(AB), U(AC), N(BC), may be represented by a triangle whose apex is A.
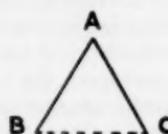
Such triangles will in fact appear on any language map. (We have, for example, UUN values for sentences with " new ", " theorem ", " bench " ; " good ", " day ", " chocolate " ; " property ", " strength ", " colour " ; " exist ", " even primes ", " bipeds ". But if such a triangle is given, then the rule R(U) prohibits the existence of another triangle having A at its base together with another expression, D, with either B or C at its apex. For example, this configuration cannot appear on the language map :
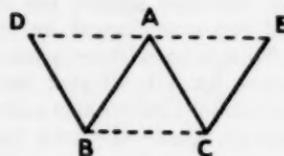
There is no expression D or E for which univocity holds at each point on this map.

The kind of structure which represents the *permissible* configurations of sense relations among the univocal expressions of

the language is a tree.[1] We omit the dotted lines for representing N-relatedness. U-relatedness exists between any two points if you can reach one from the other without having to go *both* in an upward and downward direction. If in going from one point to another you must take a route in which you travel both up and down, then those two points are N-related. For example, in the figure immediately below the point-pairs KL, HD, HL, are N-related. BH, KB, LD ᵀL are U-related.
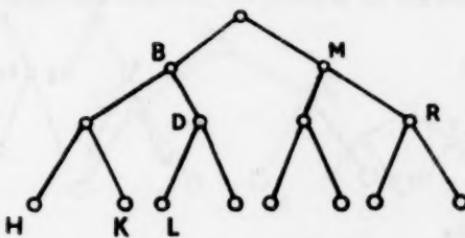
M and R are not in the same language with B, D, H, K, L since they are not U-connected with any of them. However, if we have an expression at the very top of the tree, then all the expressions are in the same language. (When an apex expression is lacking we have, in effect, more than one language map.)

This structure tells us what it means for an expression to have a given location on a map of sense relations. Moreover, if we have sentences in which we are using some expression equivocally, *i.e.* which violate R(U)—then any attempt to fit the expressions contained in these sentences onto the tree with the given sense values will fail. To take a slight variation of a previous example : let T = tall, A = argument, M = man, R = reasonable, B = building. Let the data values for some of the expression-pairs be U(TM), U(TB), U(TR), U(AR), N(TA), N(BA), N(BR), N(MB), U(MR). These values will not fit onto the tree ; we cannot give each expression a single location if these values are given, as of course they *are* in ordinary use. By splitting the sense of " reasonable " we *can* fit these values onto a tree. We simply give " reasonable " two locations.

The values given violate R(U) since we have : U(RA), U(RT) N(AT) which means that R is higher than T. UR(T), U(BT) N(BR) in which T is higher than R.

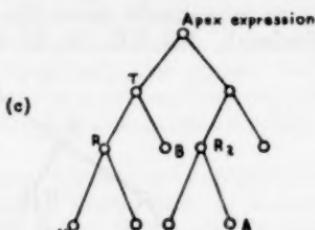[1] The topological property of a tree is that there is no more than one route between any two points.

I

Unsuccessful attempts to fit
values inconsistent with R(U)
onto the U-frame. In (a) we
get U(BR). In (b) we get
U(AT).
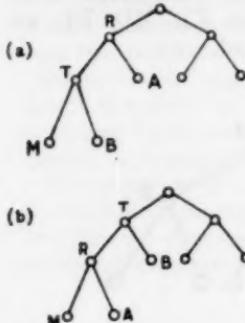
II

By splitting the sense of
R (" reasonable ") we have
U(MR₁), U(AR₂), N(AR₁),
etc. This enables us to
conform to the tree.

This is one example of how the " ordinary language tree "
enforces univocity for the expressions located on it. If an
expression has sense relations which conform to the structure
of the O.L.T. then it is univocal. Univocity *is* locatability.

The arguments of Ryle and Wittgenstein may be seen as proofs
that given certain values, the O.L.T. represents certain expres-
sions as N-related. (This is analogous to the use of Venn
diagrams to represent the premises. The conclusion is then
found already represented on the diagram.)

Take Ryle's argument. The data values are U(itch, locatable),
N(loc. mood). In the background of this argument we also have
perhaps U(serenity, mood) and N(serenity, itch). By hypothesis
the sentence, ' An itch is a mood ', is doubtful in sense value.
Thus we have D(itch, mood). If we represent the known values on
the U-frame (or tree), we have something like the diagram below.
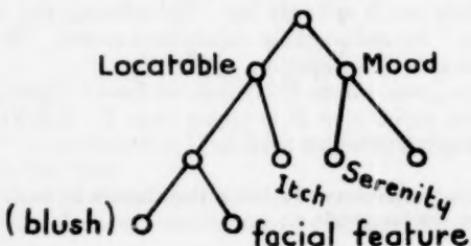
This diagram or any other which would represent the known values already represents to us the value " N " for the doubtful pair (itch, mood). If we tried to say U(itch, mood) then we could no longer represent the *known* values on the U-frame.

The same may be done for Wittgenstein's argument. Let M = mental state, C = continued all day, U = understanding a word, S = sitdown strike. By hypothesis the value of (UM) is doubtful. The data values are : U(MC) U(SC) N(SM) N(UC). Representing these values on the U-frame we see that N(UM).
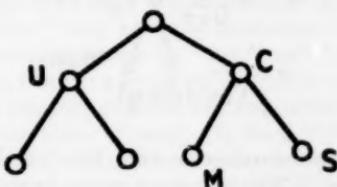
Thus the procedure for making distinctions and the procedure for deciding on doubtful sentences is essentially the same. We make a distinction whenever we are dealing with a given group of sense values which we wish to retain, but which do not fit onto the U-frame. We decide that a given sentence is nonsense when we have no stake in keeping a sense for it. In the two cases we have just examined we have a stake in keeping U(argument, rational) and this forces us to make a distinction ; we have no stake in saying U(itch, mood) ; since the argument began with that as a D-sentence we are free to consider it an N-sentence. But whether we make a distinction or whether we invalidate a sentence, the outcome is the same, *viz. all values must be such as to conform to the principle of use : they must fit onto the U-frame.*

Another familiar point is made clear by the U-frame. If, metaphorically, I wish to speak of an itch as a mood or a mood as an itch, then to be consistent in my metaphor I must also allow for the sense of " My mood was on my chest " or any other place on the body. Carrying a metaphor through consistently is nothing more than giving it those sense relations which the data sentences of ordinary language have. The use of the word may be new ; what a " use " is has not changed.

On the other hand, there is the possibility of alternate U-frames in which what counts as a use is quite different from what we have seen it to be for the natural language. Theologians have long recognized that " God " does not fit into the natural language even metaphorically. They wish to consider (God is tall) to be

an N-sentence while (God is rational) is U.   Even a metaphorical
use of "rational" does not help since, to be consistent, they
would have to allow (God is tall) that metaphorical sense which is
allowed to (God is rational).   The values U(GR), U(Man, R)
U(Man, tall), N(G, T) give us something consistent with the
U-frame :

But as soon as we introduce a word like "building" and the
values U(BT) and N(BR) we cannot retain locatability.

This and other examples point to the possibility that some
theologians may be employing another U-frame, in which the
rules for making distinctions and deciding D-sentences are
different from those for the natural languages.   There may be
poems which are built upon sense relations which are not merely
new words related in the old way (as are metaphors) but words
related in new ways on a different structural plan of sense (" use ").
Languages could be formed which violate R(U) or which prohibit
sense relations allowed by R(U).   All that is required is internal
consistency.   The U-frame is not the only possible structure of
sense relations.   It does, however, represent what we *ordinarily*
consider to be " a sense " of an expression.

If a theologian finds that his use of *God* and the predications of
God is such that these words have no sense in the way that sense
or "use " is defined by the ordinary language tree he may
work out a new sense frame.   But this situation differs from one
in which a philosopher, says Spinoza, or Whitehead, introduces
into the old tree a new sense or location for the word *God.*   The
philosopher can justify his new usage, *i.e.* he can show its locata-
bility at some point which has the required sense relations.   The
theologian has only one way of avoiding meaninglessness—to
construct a frame in which the expressions which enter into his
theological statements have a new " use ".   The philosopher's
use of " God " is a new use in the old frame (perhaps new *usage* is
here appropriate) while the theologian's use of " God " implies a
new language frame, *i.e.* one in which what counts as a use is
different from what counts as a use in ordinary language.   At the

moment of writing I find the idea of such " non-ordinary " or—
" non-aristotelian " structures for declarative sentences logically
difficult to maintain. There are certain linguistic considerations
of a broad nature which indicate that the tree-structure of
language for such sentences must go for possible as well as actual
languages. If that is correct, it has embarrassing consequences
for rational theologies which place logical restrictions on some
God words while allowing for others.

We have so far dealt with sense in the rather strict way in which
earlier we distinguished sense from meaning. It was noted that
" tall " and " short " may have the same sense but are different
in meaning ; the same is true for " tall " and " not tall ". The
reason one would rule out a sentence like " K is tall and not tall "
is not because a category mistake was committed. It is because
of other rules than those of sense. In fact, if (T, not T) *were* a
category mistake it would make no sense to call it an inconsistent
or self-contradictory sentence. A sentence which is a category
mistake cannot get to be contradictory. One may insist on this
against any view which considers that inconsistent sentences
violate *two* rules, type rules and rules governing the affirmation
and denial of predicates. It can in fact be shown that all expres-
sion-pairs of the form (X, not X) are U in sense value.[1]

Inconsistency is therefore an added prohibition dependent
upon the non-violation of category rules defined by the tree
structure. This is one example of rule dependence or rule
priority in language. All absurdity is the result of the violation
of some rule. And you cannot violate a rule unless you have
adhered to those rules which obtain as a condition for this rule
being in effect. In games this is easily seen since, there, rule
priority is also temporal. You cannot be tagged out at third
unless you have adhered to rules about proper running in base
paths, not being hit by a struck ball, not having been forced at
second, etc.

What has just been said about the sense value of inconsistent
sentences is equally true of the so-called antinomies. Here great

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[1] For any expressions X, Y, Z, W, the rule prohibiting conflicting triads
gives us (1) $\sim$ [U(XY)U(XZ)N(YZ) · U(XY)U(YW)N(XW)]
and (2) $\sim$ [U(XY)U(XZ)N(YZ) · U(XZ)U(ZW)N(XW)]
from (1) we get (1.1) N(XW)[U(XY)UXZ)U(YW)] $\supset$ U(YZ)
from (2) we get (2.1) N(XW)[U(XY)U(XZ)U(ZW)] $\supset$ U(YZ)
These state a sufficient condition for the U-relatedness of two expressions.
If we substitute for Z, not Y, we have : N(XW)[U(XY)U(X$\sim$Y)U(W2Y)
$\supset$ U(Y,$\sim$Y)
For example let X = Man, W = Building, Y = tall
N(Man, Building)[U(Man, tall)U(Man, not tall)U(Building, not tall) $\supset$
U(tall; $\sim$ tall).

damage has been done to the cause of linguistic clarity by placing the antinomies under " category mistakes " and considering a sentence like " I am a liar " to be the same sort of absurdity as " Wednesday is awake " only that in the latter case the absurdity is obvious while in the former it has to be proven or exhibited.

The unfortunate use of the concept of " type " as applied to antinomies has obscured the difference between the sorts of mistake embodied in these two sentences. A true type mistake is one in which there are conjoined two expressions which are N-related on a map of sense relations. It is easy to show that the liar's statement does not combine N-related expressions : Assume that the liar's statement is an N-statement and also, as Ryle assumes, that the sentence " The writer of this paper is one who never wrote a paper " is the same sort of type error as " I am one who is now lying ". Let us symbolize the first sentence by (W, NW) and the second by (I, L). (According to the traditional view, they both are N-sentences committing the same type of mistake as, say, " Anger is a square ".) Now ordinary usage supplies the following data sentences : U(I, W) U(I, NW) U(L, W), U(L, NW). This gives us a violation of the rule of use since we have two triads U(I, W) U(I, NW), N(W, NW) and U(I, W), U(L, W), N(I, L). In the first triad we have " I " higher than W, in the second W is higher than I.

Another proof would use the above theorem (in footnote 4) which gives us : $N(W, NW) \cdot U(IW) \cdot U(LW) [U(I, NW) \lor U(L, NW)] \supset U(IL)$. This shows that either (I, L) is a U-sentence or (W, NW) is a U-sentence. But in any event there is no reason to consider the liar's statement or the writer's statement as category mistakes. All linguistic evidence is on the side of considering these sentences to make sense in the way we define sense—by the criterion of the U-frame. If we adopt N-values for these sentences we can no longer locate them. And if we distinguish two senses for " I " in order to locate that word, then in one of them (the " ordinary " sense of " I ") the sentence " I am a liar " will make sense anyway. Thus, as far as the type is concerned such sentences are good. If they are no good (and I do not for a moment wish to validate them in any other way) they are ruled out apart from type rules. Moreover, whatever rule is used to invalidate them is in effect precisely because they make type sense.

One last polemical consideration for the view that inconsistent sentences and antinomies are category correct is that wherever it makes sense to use A it makes sense to use not-A. The exception, of course, is the identity sentence " A is A " where a substitution

for one of these renders the sentence invalid. The question then arises whether it is not simpler to consider A and not-A to be expressions of the same type and to recognize a special rule which invalidates the resulting self-contradiction, or whether to consider A and not-A to be of different types. I have shown that the latter course is not really defensible.

Some final remarks in summary : If I am correct in saying that the U-frame is the correct representation of the sense relations obtaining among the expressions of the natural language, then one important consequence is the possibility of a formal treatment of a good part of the logic of ordinary language. *Contra* Ryle, there *are* " stereotyped operations " and " regulation moves " to be made in what he has called " informal logic ".

Metaphysical expressions, which some philosophers call meaningless and others—bowing to usage—call mysterious, appear to be neither. Any word which has location in the U-frame has, to that extent, meaning. Definition is location ; at least nominal definition always is. If metaphysical words are meaningless they can only be ruled out because of some violation of other rules than those which give *sense* to expressions. Not only are metaphysical words locatable in the natural language ; they are absolutely necessary. The tree needs an apex. This is a structural requirement of our language. Any apex word is U-related to all the other words of a given language. This fact renders it suspicious since such words *have no misuse*. Their sense is, however, just as unique as the sense of any other expression on the U-frame. They, like any other expression, have only one location and it ought not to be forgotten that this location is given them by the mutual sense relations given them by the expressions of the language. An expression like " can be talked about " is a predicate for any other expression in the language. So, too, in one of its meanings, is " exists ". There is no reason to consider these expressions " equivocal " any more than there is reason to consider " can be talked about " equivocal just because it can be said of " man " and " tree ". It is, however, true that in some sense, metaphysical predication gives very little information. But that has to do with truth, not with sense. And that is another topic.

It remains true that distinctions and sense values will vary according to linguistic contexts. In particular, if more expressions are added to any tree of sense relations, there will be more distinctions needed to retain univocity for all the expressions. Also a very interesting phenomenon occurs. We have empty locations on the tree for which we have no words. Yet these

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locations are concepts which need only to be named. The construction of a tree opens up the possibility of numbering locations and giving names to these concepts.

The construction of other U-frames than the one now in use is another interesting possibility.

ADDENDUM

A simple notation which enables us to tell at a glance whether a given point is U or N-related to another given point is a desideratum. The tree which follows uses " left " and " right " for a binary notation. The numbers " 1 " and " 0 " would do just as well.
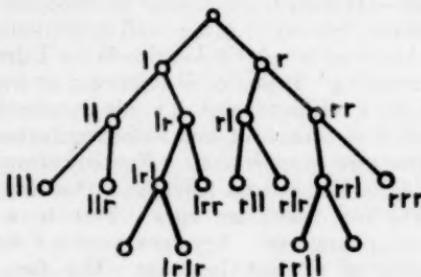
With this notation any two points are U-related only if one has more digits than the other and the one with the smaller number of digits is identical with the beginning of the one with the larger number of digits. Thus lrll is U-related to lrlllrl. On the other hand, lrll is N-related with lrlrlrl. The number of points at the nth level of the tree is 2^n. The number of digits at any point is n-l. It can be shown that the introduction of a new expression onto a tree requires a maximum of n decisions or bits of information. This is for a tree with n levels.

The tree may be read as a diagram for total class inclusion—a scheme in which the inclusion of part of a class in another class is disallowed. Thus the metaphysical apex word at the top includes the two small circles " l " and " r " which do not overlap. Each of these then includes, respectively, two circles, which again do not overlap and so on. What is significant is that this algebra may be interpreted as a sense calculus. The reason for this is that while *truth* depends upon partial inclusion (*e.g.* " Only some men are six feet tall "), sense requires that the sentence " All men are six feet tall " be significant before we can allow " Some men are

six feet tall ". With the sense it is all or nothing. Thus, quantification is only important in a truth calculus. The sentence " No men are six feet tall " makes sense only if *any* quantification is to make sense. Hence this sentence would also imply the total inclusion of the subject in the predicate.

A " branch " of the tree is constituted by those expressions which are mutually U-related, *e.g.* l, lr, lrl, lrll, lrllr, etc. Any such branch constitutes a type and the expressions belonging on a branch are expressions of the " same type ". This definition of " type " seems to me to be preferable to the usage which restricts sameness of type to a single location by the requirement that the use and misuse of two expressions be identical.

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III.—ARISTOTLE AND SYLLOGISMS FROM FALSE PREMISSES

By G. Patzig

In spite of Lukasiewicz's great book on the subject it is still far from being settled if it makes, after all, even sense to discuss Aristotle's syllogistic "from the standpoint of modern formal logic". One of the moot points, as is now well known, is the question whether Aristotle's syllogisms are rules of inference or logical theorems. Traditional logic formulated the syllogisms as rules: MaP, SaM, *therefore* SaP. Russell and Whitehead (*PM*, I, p. 28) have called this a "slipshod way of speaking" as it gives the impression of both premisses and conclusion being *asserted*. Textbooks of logic do not usually say what happens if the premisses in question are false. It might puzzle us for a moment to notice that none other than G. Frege, in a letter to Jourdain from 1910, has put forward the view that "aus falschen Prämissen kann nichts geschlossen werden". But, with Frege, "schliessen" is *defined* as the application of his "Abtrennungs-regel", and therefore one cannot really say he would endorse the traditional view.

Now Lukasiewicz has tried to show that the syllogisms of Aristotle are not, like those of traditional logic, *rules* of inference, but genuine *theorems* of logic. His strongest argument is that Aristotle in his systematic discussion of syllogisms and figures (Anal. pr. A 4-7) introduces his syllogisms in the form of conditional sentences with both premisses as antecedent and the conclusion as consequent, *e.g.* "If A is predicated of all B and B of all C, then it is necessary that A should be predicated of all C" (A 4, 25 b 37). This thesis of Lukasiewicz's has been contested on different grounds by various scholars. It is called a "strange contention" by Professor Prior.[1] According to him, Aristotle does not, in these chapters, *formulate* syllogisms—he just *talks about* them, and if this is so, his way of talking would be "perfectly natural". It certainly would not imply any differences between the Aristotelian and the traditional syllogism. A German writer, W. Albrecht, takes another line of approach. Lukasiewicz's thesis cannot be true, he says, because Aristotle never recognised syllogisms from false premisses. Now, since a theorem of the form: "*If* MaP and SaM, *then* SaP" would hold

[1] A. N. Prior, "Lukasiewicz's Symbolic Logic", *Australasian Journal of Philosophy* xxx (1952), 33–46.

even for those terms that do *not* make the premisses true, such a theorem cannot be identified with the Aristotelian syllogism.[1] For his opinion that Aristotle did not think syllogisms with false premisses valid, Albrecht could have claimed the support of H. Maier, who (in his " Syllogistik des Aristoteles ", II, 2, p. 246) asserted that in syllogisms with one or two false premisses, according to Aristotle, " the conclusion does not at all issue from the premisses with syllogistic necessity ". Maier also has a predecessor : he follows Th. Waitz, who summarised the same passage which Maier had before him in his commentary from 1844 like this : " Explicatur cur conclusio vera, quamquam fieri possit e falsis propositionibus, tamen non necessario ex iis proveniat." Albrecht's view, we must concede, can boast of being founded on some well-established interpretation of a crucial passage of the Organon's text.

I shall for the rest of this paper not comment on Professor Prior's interesting remarks, since they are best discussed along with some other questions he raises in his review of Lukasiewicz's book. For the present I shall concentrate on what might be called the Albrecht argument. I shall try to show by an analysis of the relevant passages (Anal. pr. B 2, 53 b 4-10 and B 4, 57 a 36-b 17) that Aristotle's actual teaching is squarely opposed to the Albrecht, Maier and Waitz interpretation. The analysis of these passages seems interesting also on other grounds : it gives a fair idea of the technical skill of Aristotle's logical discussions and, at the same time, we shall find that Aristotle has a fallacy in his main argument. That fallacy has already been detected by Lukasiewicz [2] and it seems worth while, since Sir David Ross's otherwise excellent commentary is not of much help here, to present the argument in its proper context.

Aristotle starts (53 b 3-10) with the remark that both premisses of a given syllogism may be true propositions, but that they might as well be both false or of differing truth value. The conclusion is, he says, necessarily either true or false. Now it is impossible to infer a false conclusion from true premisses ; but there may be cases in which we can infer a true conclusion from false premisses. But, he adds, such a syllogism can only be a syllogism of the fact, not one of the reason. This he proposes to deal with somewhat later on (ἐν τοῖς ἑπομένοις, 53 b 10). The reference is here, as in Anal. pr. A 1, 24 b 14, definitely to Anal. post. A 2, because only in that chapter Aristotle points out that demonstrative syllogisms which state (in the premisses) the

[1] W. Albrecht, *Die Logik der Logistik* (Berlin, 1950), especially pp. 56 ff.
[2] Aristotle's *Syllogistic* (Oxford, 1951), p. 49.

reason of the fact asserted by the conclusion, *must* have *true*
premisses. H. Maier and Sir David Ross, who both think the
reference is to the second of our passages (57 a 36-b 17), have
mainly on account of this initial mistake precluded themselves
from understanding Aristotle's argument.

Aristotle continues (53 b 11-15) with some reflections that are
destined to show that from true premisses only true conclusions
can follow. If, he says, from the proposition A follows a proposi-
tion B, then also Non-A follows from Non-B. (This is Theorem
11.61 in Bochenski's *Ancient Formal Logic*, Amsterdam 1951.)
Since Aristotle has stated shortly before (Anal. pr. A 46, 52 a 32 ff.),
the equivalence between the statement of a fact and the statement
that the sentence expressing the fact is true, he is justified in
inferring the proposition that, if A implies B, the truth of A
implies the truth of B. Now the rest is plain sailing : For A in
this formula Aristotle substitutes the two premisses of a syllogism
(53 b 23 ; the same substitution we find in Anal. pr. A 15, 34 a 22),
for B he substitutes the conclusion. The truth of the con-
clusion follows logically, then, from the truth of the premisses.
For if we suppose B to be not true, then, according to our for-
mula, we could infer the falsity of A, the conjunction of the
premisses.

It is, however, possible to infer true conclusions from false
premisses, but not in all the syllogistic moods. These are now
examined by Aristotle in this respect : According to " Barbara ",
e.g. we can infer the true proposition " All men are animals "
from the false premisses, " All men are stones, All stones are
animals ". After these not very interesting detailed investigations
Aristotle continues his general discussion in the second of the
passages we want to analyse (57 a 36-b 17). It is evident, he
says, that at least one premiss of a correct syllogism must be false
if the conclusion is to be false. On the other hand it is not, as one
might be tempted to think, necessary that at least one premiss of a
syllogism with a true conclusion be *true*. It is, actually, quite
possible that the conclusion is true even when both the premisses
are false—οὐ μὴν ἐξ ἀνάγκης, but there is no necessity for
this. No necessity for what ? Obviously for the conclusion
being a *true* sentence, not for the conclusion *following* from the
premisses, as H. Maier understands this all-important clause.
The whole passage does not deal with the question if the conclu-
sion follows in such cases, still less with the problem if the
conclusion " follows by necessity " or " simply follows "—which
is what Maier actually thinks Aristotle says (*loc. cit.* p. 246). A
conclusion is, for Aristotle, *defined* as a proposition that follows

from certain premisses with necessity : τὸ μὲν . . . γὰρ συμβαῖνον ἐξ ἀνάγνης τὸ συμπέρασμά ἐστιν (53 b 18).

It is, then, quite off the mark to say, as H. Maier does, that Aristotle failed, in this passage, to distinguish properly between the truth (*i.e.* the validity) of a syllogism and the truth of the propositions which constitute it.   Rather it is H. Maier who has failed to notice a distinction which Aristotle actually emphasises : Aristotle does not say a true conclusion could not follow with necessity from false premisses, but he says and teaches, consistently, that a conclusion, which follows from false premisses, is not necessarily *true*.   That these are two different propositions is clear from the fact that one of them is false, the other true.   It is, of course, an overstatement to say that H. Maier did not see the difference between both these propositions, since he never even thought our second proposition a possible interpretation of the text in question.   But Sir David Ross regards them, clearly, as equivalent.   He says : " The main thesis is that in such a case " (false premisses), " the conclusion does not follow by syllogistic necessity (a 40).   This is, of course, an ambiguous statement.   It might mean that the truth of the conclusion does not follow by syllogistic necessity, but if Aristotle meant this, he would be completely contradicting himself.   What he means is that in such a case the premisses cannot state the ground on which the fact stated in the conclusion actually rests, since the same fact cannot be the necessary consequence of another fact and of the opposite of the other." [1]   Sir David asserts, consequently, that Aristotle shows in 57 a 40-b 3 that the conclusion actually follows, and in b 4-b 17, that the premisses cannot here state the ground on which the fact asserted by the conclusion rests.   But we do not find any demonstration of this kind whatever in the text : that a conclusion " actually follows "—if it be a conclusion —is for Aristotle a tautology and does not need any demonstration. And that false premisses do not provide a reason for their true conclusion is certainly an opinion of Aristotle's, but one that has left not even a trace in b 4-17.   Sir David has simply been misled by Aristotle's remark in 53 b 7 that he would discuss that point later on, as I said before.   I confess to being unable to understand the " since " in the quoted passage of Sir David's commentary ; I do not see how the proposition that one fact cannot be the necessary consequence of another fact and the opposite of this other fact could ever be a *proof* of the proposition that false premisses cannot give the reason for the fact stated in the conclusion.   For there is certainly no necessity for the false premisses

[1] W. D. Ross, *Aristotle's Prior and Posterior Analytics* (Oxford, 1949), p. 436.

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premisses, but not in all the syllogistic moods. These are now
examined by Aristotle in this respect : According to " Barbara ",
e.g. we can infer the true proposition " All men are animals "
from the false premisses, " All men are stones, All stones are
animals ". After these not very interesting detailed investigations
Aristotle continues his general discussion in the second of the
passages we want to analyse (57 a 36-b 17). It is evident, he
says, that at least one premiss of a correct syllogism must be false
if the conclusion is to be false. On the other hand it is not, as one
might be tempted to think, necessary that at least one premiss of a
syllogism with a true conclusion be *true*. It is, actually, quite
possible that the conclusion is true even when both the premisses
are false—οὐ μὴν ἐξ ἀνάγκης, but there is no necessity for
this. No necessity for what ? Obviously for the conclusion
being a *true* sentence, not for the conclusion *following* from the
premisses, as H. Maier understands this all-important clause.
The whole passage does not deal with the question if the conclu-
sion follows in such cases, still less with the problem if the
conclusion " follows by necessity " or " simply follows "—which
is what Maier actually thinks Aristotle says (*loc. cit.* p. 246). A
conclusion is, for Aristotle, *defined* as a proposition that follows

from certain premisses with necessity : τὸ μὲν . . . γὰρ συμβαῖνον ἐξ ἀνάγνης τὸ συμπέρασμά ἐστιν (53 b 18).

It is, then, quite off the mark to say, as H. Maier does, that Aristotle failed, in this passage, to distinguish properly between the truth (*i.e.* the validity) of a syllogism and the truth of the propositions which constitute it.   Rather it is H. Maier who has failed to notice a distinction which Aristotle actually emphasises : Aristotle does not say a true conclusion could not follow with necessity from false premisses, but he says and teaches, consistently, that a conclusion, which follows from false premisses, is not necessarily *true*.   That these are two different propositions is clear from the fact that one of them is false, the other true.   It is, of course, an overstatement to say that H. Maier did not see the difference between both these propositions, since he never even thought our second proposition a possible interpretation of the text in question.   But Sir David Ross regards them, clearly, as equivalent.   He says : " The main thesis is that in such a case " (false premisses), " the conclusion does not follow by syllogistic necessity (a 40).   This is, of course, an ambiguous statement.   It might mean that the truth of the conclusion does not follow by syllogistic necessity, but if Aristotle meant this, he would be completely contradicting himself.   What he means is that in such a case the premisses cannot state the ground on which the fact stated in the conclusion actually rests, since the same fact cannot be the necessary consequence of another fact and of the opposite of the other." [1]   Sir David asserts, consequently, that Aristotle shows in 57 a 40-b 3 that the conclusion actually follows, and in b 4-b 17, that the premisses cannot here state the ground on which the fact asserted by the conclusion rests.   But we do not find any demonstration of this kind whatever in the text : that a conclusion " actually follows "—if it be a conclusion —is for Aristotle a tautology and does not need any demonstration. And that false premisses do not provide a reason for their true conclusion is certainly an opinion of Aristotle's, but one that has left not even a trace in b 4-17.   Sir David has simply been misled by Aristotle's remark in 53 b 7 that he would discuss that point later on, as I said before.   I confess to being unable to understand the " since " in the quoted passage of Sir David's commentary : I do not see how the proposition that one fact cannot be the necessary consequence of another fact and the opposite of this other fact could ever be a *proof* of the proposition that false premisses cannot give the reason for the fact stated in the conclusion.   For there is certainly no necessity for the false premisses

[1] W. D. Ross, *Aristotle's Prior and Posterior Analytics* (Oxford, 1949), p. 436.

in question to be the *negations* of those premisses which contain, in Aristotle's sense, the " ground " of the conclusion.

Aristotle does not, in these passages, discuss the question whether in a syllogism with false premisses the conclusion follows with necessity. Still less does he decide that it does not, as H. Maier would have us think. Neither does he deal with the problem whether false premisses can contain the ground of the fact asserted by the conclusion, as Sir David Ross supposes. The problem Aristotle actually talks about is this : how is it that, while from two true premisses invariably a true conclusion follows and, if the conclusion is false, one of the premisses *must* be false, a conclusion from two false premisses or from premisses differing in truth-value may nevertheless be either true or false, just as it may happen ? This is the logical puzzle Aristotle tries to understand, and he thinks he can offer a proof that this *must* be as it is. His argument runs like this : In no case can one fact be the necessary consequence of some other fact and the opposite of this fact as well. Therefore, precisely *because* from the truth of the premisses the truth of the conclusion results by necessity, it is impossible that also from the falsity of one or both the premisses the truth of the conclusion should follow by necessity. Therefore the conclusions of syllogisms with false premisses are propositions that are by necessity not necessarily true. The same argument is applicable to the truth—or—falsity of the premisses : If the falsehood of the conclusion implies the falsity of at least one premiss, it is impossible that also from the truth of the conclusion the falsity of at least one premiss results by necessity. Therefore the conjunction of the premisses in a syllogism with true conclusion would by necessity be not necessarily false.

All this would in fact be proved if the proposition were shown to be true that one and the same fact *cannot* follow from another fact and its opposite. Now our passage 57 b 3-17 contains nothing but Aristotle's attempt to give a demonstration of this principle. This is the only way to understand the text, but it is clearly a way to understand the text most thoroughly. The proof given by Aristotle is subtle, even elegant, but fallacious—as has already been pointed out by Lukasiewicz. The absurdum to which Aristotle reduces the hypothesis of the principle's being untrue looks absurd enough, but is not.

Aristotle proceeds like this : for the proposition " Both premisses are true " he substitutes the proposition " A is white ". For the proposition " Not both the premisses are true " (the negation of the first proposition) he puts the proposition " A is not white ". The proposition " The conclusion is true " is substituted

by the proposition " B is large ", and the proposition " The conclusion is false " by the proposition " B is not large ".

Aristotle then gives two logical laws : (A) " If A implies B, then Non-B implies Non-A " (57 b 1 ff.) and (B) " If A implies B and B implies C, then A implies C " (57 b 6-9). Both (A) and (B) are elementary rules of propositional logic. Now if the fact that B is large (the conclusion is true) could *follow* from the fact that B is white (both premisses are true) and *as well* from the fact that B is not white (not both premisses are true), we would have the two implications (1) " From ' A is white ' follows ' B is large ' " and (2) " From ' A is not white ' follows ' B is large ' ". From (1) we get, according to (A), the implication (1') " From ' B is not large ' follows ' A is not white ' ". Now we apply (B) to (1') and (2) and we get the new proposition (3) " From ' B is not large ' follows ' B is large ' "—and this is evidently absurd, says Aristotle (τοῦτο δὲ ἀδύνατον, 57 b 14). And H. Maier (*loc. cit.* II, 1, p. 331) and also Sir David Ross (*loc. cit.* p. 437) quite agree with him on this point. Both scholars think a proposition of the form " If Non-A, then A " to be self-contradictory. But, of course, only propositions of the form " Non-A *and* A " are self-contradictory, and a conjunction of two propositions is not equivalent to the conditional of these propositions, not even in Aristotle's logic. One is always tempted to think a proposition absurd when we cannot imagine a situation in which it would be the natural thing to utter this proposition. But that we cannot think of such a situation does not imply the absurdity of the proposition itself. On the other hand there may well be situations in which it is quite sensible to utter absurd propositions—examinations being one ready case of this. So the attempted proof of the principle that one fact cannot follow from another fact and the opposite of this fact breaks down here.

But it can also be shown that the principle itself is not true. There are cases which justify statements that openly violate Aristotle's principle. It is unfortunately true of some patients that they will die if operated upon and also when no operation will take place, *e.g.* in cases of advanced appendicitis complicated by extreme weakness of heart. And in such cases, apart from all general problems of causality, there is certainly a fact and its opposite the " ground " for another fact—at least in exactly that sense Aristotle wants to rule out by his principle. For we certainly can apply Aristotle's rules (A) and (B) to our example and we will get the proposition " If the patient does not die, he will die "—which is only a rather pointless way of saying he will die anyway.

Aristotle has, to sum up, been puzzled by the logical fact that a conclusion correctly inferred from false premisses is neither by necessity true nor false. He has, to overcome his difficulty in understanding this simple fact, tried to devise a proof of it. But he failed : the main premiss of his proof (what I called his " principle ") is false, and the attempted demonstration of this principle by *reductio ad absurdum* is fallacious. Now I think it is easily understood that Aristotle should feel puzzled by the logical fact that puzzled him. He freely uses rules of propositional logic in his systematic account of the syllogistic moods and figures, but he never stopped to analyse these logical structures he made use of. The truth-table-like treatment of the conditional by the stoics somewhat later on showed that there is no puzzle here at all.

We have seen that Aristotle does not deal in our passage with the question whether syllogisms from false premisses can be valid nor with the problem whether false premisses can state the ground of the fact asserted in the conclusion. He talks simply about the relations that hold between the truth-values of premisses and conclusions in valid syllogisms, and he states them correctly. The proof he offered for the logical fact correctly stated by him was, however, shown to be incorrect. Now the interpretation of H. Maier is curiously different from the one just given : H. Maier thinks Aristotle wants to establish quite a different thesis and one that even Maier himself would not accept. But the proof Aristotle gives for this thesis Maier regards as correct. Yet it is certainly somewhat odd to reject a thesis and still to accept a proof for this thesis as correct. The interpretation given by Sir David Ross is at least self-consistent. But Sir David, too, accepts a formally incorrect proof as valid confirmation of a thesis the proof could not establish even if it were correct.

I conclude, then, that the Albrecht argument against Lukasiewicz's thesis has no foundation in the text of the Organon.

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IV.—MUST THE FUTURE BE WHAT IT IS GOING TO BE?

By R. D. Bradley

ALTHOUGH logical determinism, correctly understood, simply asserts some logical truisms about past, present, and future events and states of affairs, it has often been confused with a very different view, *viz.* fatalism, and so has been made to share, undeservedly, the disrepute of the latter. Let us see how this has come about.

If we use the symbols x and F as variables whose values respectively are the logical subjects and logical predicates of propositions, we may say that logical determinism simply asserts of every proposition that:

(1) Fx entails Fx (law of identity);

(2) 'Fx and not-Fx' is logically impossible (law of non-contradiction); and

(3) either Fx or not-Fx must be true (law of excluded middle).

That is to say, for any x and any F, if F can significantly be predicated of x, Fx must be either true or false; if Fx is true, then not only must what Fx asserts be the case, but it is logically impossible that it should *not* be the case (since otherwise Fx would not be true); and if Fx is false, then not only must what Fx asserts not be the case, but it is logically impossible that it *should* be the case (since otherwise Fx would not be false). Thus, according to logical determinism, everything is determined or rather *determinate* in the sense that it is what it is and logically cannot be otherwise. And this holds of past and future states of affairs as well as present ones. The past was what it was and could not have been different; it is as though its record is indelible, for if an event occurred in the past there is nothing that anyone (even God) can do to erase the fact that it has occurred or to bring it about that it has not occurred. In this sense, the past is closed; it cannot be tampered with or altered by any means whatever. In the same sense the future too is closed. For it is a logically necessary truth that what will be will be; if an event is to occur, there is nothing and no-one that can conceivably bring it about that it will not occur; even God is impotent in the face of logical necessity. You cannot make the future other than it will be any more than you can make the past other than it was.

What disturbs many people about this is that, despite its apparently unquestionable logical credentials, logical determinism seems to deny some obvious and important truths. Certainly, on the one hand, it *looks* like a tautology to say, for example, that what is to be will be, but on the other hand, this apparent tautology seems to imply that all activity is fruitless, that there is, for instance, no point in obtaining medical advice and attention even when dangerously ill since, if the patient is going to recover he might as well dispense with the doctor's services, and if he is not going to recover no amount of skill or attention on the doctor's part will make any difference. There are, of course, some religious sects whose adherents strenuously oppose medical aid when they and their children are ill on grounds rather like these, *e.g.* " God knows best " and " God's will be done ", but most of us regard this sort of attitude as wrongheaded and perhaps, in cases where it leads to serious ill-health or death, even criminally reprehensible. That is, we deny that all effort and activity is pointless and hence are committed to denying whatever statement or statements fatalism is implied by. The argument is that if logical determinism implies fatalism and logical determinism is necessarily true, then fatalism must be true ; but we all agree that fatalism is false, therefore it must be false either that logical determinism is necessarily true or that logical determinism entails fatalism. We want to know which.

To put it a little differently, we are faced with the following inconsistent triad :

(1) Logical determinism is true ;
(2) Fatalism is false ;
(3) Logical determinism entails fatalism.

We accept (2) but must reject either (1) or (3) since each of them, in conjunction with (2), implies the contradictory of the other. The issue at stake is whether belief in logical determinism is warranted. Thus, on the one hand, determinists accept (1) and (2) and so conclude that (3) is false ; that is, they believe both that logical determinism is true and that fatalism is false, but recognise that they can do so if and only if logical determinism does not entail fatalism. On the other hand, however, many libertarians accept (3) and (2) and so conclude that (1) is false ; that is, they use the supposed fatalist implications of logical determinism in order to discredit logical determinism. The question to be decided, then, is this : " Does logical determinism involve fatalist conclusions ? "

I propose to examine, first, the consequences of, and secondly, the purported reasons for the affirmative answer to this question.

I

Some libertarians think that logical determinism *is* fatalistic and this, I believe, is what leads them to deny that the law of excluded middle is applicable to statements about future events and states of affairs, and thereby to deny also that the laws of identity and non-contradiction apply to them either.   Their argument, as we have seen, takes the form :   " (3) and (2), therefore not (1)." This form of libertarianism holds that future events are logically contingent since neither they, nor statements about them, are subject to the above-mentioned logical laws.   They are contingent, not in the sense in which to say that the future is ' contingent ' is merely to say euphemistically that we are ignorant about the future, but rather in the sense of *real* contingency, *i.e.* of being in themselves indeterminate.   Of such events it cannot be said either that they will or will not occur, nor can it be said that statements about them are either true or false.   As Aristotle put it :   " when in future events there is a real alternative, and a potentiality in contrary directions, the corresponding affirmation and denial have the same character ",[1] *i.e.* statements about them are only potentially true or false but are actually neither.

Clearly, this involves the rejection of ordinary two-valued logic according to which every proposition must, by definition, be either true or false ; or rather, it involves the *restriction* of the area of application of two-valued logic and hence also of the laws of excluded middle, non-contradiction and identity.   That is to say, only those statements which are about present and past states of affairs can be said to be either true or false since they alone refer to determinate facts the knowledge of which enables us to assess their truth-values ; when we purport to be talking about the future our presumptive statements can be neither true nor false since, unless we accept fatalism, we cannot allow that the future contains any determinate facts to which they can refer and by reference to which their truth-values can be ascertained.   Strictly speaking, of course, it is improper, on this view, to talk about either " statements in the future tense " or " propositions about the future " for, according to present-day usage, a sentence which does not assert or deny that something determinate is the case is not a statement any more than a sentence which is neither true nor false can express a proposition. However, there is an established precedent for using these terms

[1] *De Interpretatione*, chapter 9 (19a 33).

rather more loosely, *i.e.* in such a way that it is *not* self-contra-
dictory to speak of statements or propositions which are neither
true nor false.   Aristotle, for instance, spoke in this sort of way
when this so-called ' problem of present truth and future contin-
gents ' was first discussed.[1] Similarly, proponents of three-valued
logic talk about ' propositions ' (about contingent future events)
which have neither the determinate value ' 1 ' (true), nor the
determinate value ' 0 ' (false), but the indeterminate value ' $\frac{1}{2}$ '
(neuter or possible).

The interesting thing is that quite a few philosophers have
actually resorted to three-valued logic principally as a means of
escape from the kind of logical fatalism to which they think the
law of excluded middle and ordinary two-valued logic commits
us.   This is clearly recognised by Prior who recently suggested
that the oddity of some of the features of three-valued logic
largely disappears if we " relate the system to the problem which
it was originally designed for handling—the problem of ' future
contingents ' ".[2]   Again, according to Waismann, the argument
from fear of fatalism was " actually propounded by Lukasiewicz
in favour of a three-valued logic with ' possible ' as a third truth-
value alongside ' true ' and ' false ' ".[3]   Apparently, then, the
confusion of logical determinism with logical fatalism has gener-
ated not only the doctrine of real counter-logical contingency but
at least some three-valued logics.

It would be wrong, however, to think that everyone who
claims to have renounced the law of excluded middle or adopted
a system of three-valued logic is a logical libertarian, for on at
least some interpretations of what it means to say that a propo-
sition is ' true ' or ' false ' this certainly does not follow.   For
instance, Professor C. D. Broad is able to remain largely uncom-
mitted on the determinism *versus* libertarianism issue despite the
fact that he has argued that propositions which profess to be
about the future are not literally true or false since they " do
not refer to any fact . . . at the time when they are made ",[4]
even although " it is possible for anyone who understands their
meaning to see what kind of fact *will* eventually make them true
or false as the case may be ".[5]   Again, anyone who held that a
proposition could be said to be true if and only if it is capable of
immediate or direct verification would have to say that the law

[1] *De Interpretatione,* chapter 9.
[2] " Three-Valued Logic and Future Contingents ", *Phil. Quart.* (1953),
p. 322.
[3] " How I See Philosophy ", *Contemp. Brit. Phil.,* 3rd ser., p. 456.
[4] *Scientific Thought,* p. 73.
[5] *Ibid.* p. 77.

of excluded middle did not apply to propositions about the future, but he would not, or need not, say this in a sense which is inconsistent with the thesis of logical determinism. In such cases it is important to distinguish between saying that future states of affairs are indeterminate with respect to our knowledge and saying that they are indeterminate in themselves, or again between saying that propositions about the future are not *known* to be true or *known* to be false and saying that they are in fact neither true nor false. So long as the future is indeterminate only in a relative or epistemic sense these assertions are quite consistent with logical determinism. Similarly, there is no inconsistency between the principle of excluded middle as it is usually understood and three-valued logics so long as the terms ' true ' and ' false ' as they occur in the latter are given interpretations different from those that we ordinarily give them.

Lukasiewicz is far from being the only philosopher to suppose that the laws of excluded middle, identity and non-contradiction, when applied to the future, carry with them fatalist consequences. Other examples, too, may readily be cited. Aristotle for instance, concludes his discussion of the problem of future contingents by observing : " It is therefore plain that it is not necessary that of an affirmation and a denial one should be true and the other false. For, in the case of that which exists potentially but not actually, the rule which applies to that which exists actually does not hold good." [1] This, he argued, followed from the fact that the contrary supposition that it is an " irrefragable law that of every pair of contradictory propositions . . . one must be true and the other false " leads to the " impossible conclusion " that there could be " no real alternatives ", that everything that is or takes place would be the " outcome of necessity ",[2] and that there would therefore be " no need to deliberate or take trouble ".[3] True, Aristotle thought that he could at the same time safeguard the law of excluded middle by allowing that the entire disjunction " Either there will be a sea-battle tomorrow or there will not " is necessarily true although neither of its component propositions, " There will be a sea-battle tomorrow " and " There will not be a sea-battle tomorrow ", is either true or false, but here he was obviously mistaken. As Professor D. C. Williams comments : " This is a downright contradiction, not to be saved by the traditional apology that S or *not*-S can be ' indeterminately ' true while neither S nor *not*-S is ' determinately ' true." [4] Or, to put it in another way : the disjunction ' Either p or not-p '

[1] (19b 1).   [2] (18b 26).   [3] (18b 32).
[4] " Professor Linsky on Aristotle ", *Phil. Review* (1954), p. 253.

is a truth-function which is true if and only if one of its components, ' p ' or ' not-p ', is true.   A recent example is Professor A. N. Prior who, according to his pupil, R. J. Butler,[1] reasons thus :

> " Belief in free-will is warranted ;
>> Belief in free-will entails the adoption of a logic admitting propositions neither true nor false ;
>> Therefore, the adoption of a logic admitting propositions neither true nor false is warranted."

Even determinists have sometimes failed to distinguish between logical determinism and fatalism and so have incautiously renounced the former because of the latter.   Thus, Nowell-Smith has written about " logical determinism or fatalism, according to which we cannot help doing what we do since things are what they are and our actions will be what they will be ", and observed : " I shall forbear to comment on this brand of determinism, since I doubt whether anyone is now seriously deceived by it."[2] Similarly Waismann uses the expressions " logical determinism ", " fatalism ", and " logical predestination " as if they all meant exactly the same.[3]   Now certainly fatalism, *viz.* the view that " we cannot help doing what we do ", is, as Nowell-Smith correctly says, one of the " bogeys that go under the name of ' determinism ' "[4] and needs therefore to be exorcised, but logical determinism, *viz.* the claim that " things are what they are and our actions will be what they will be ", is not : rather is it presupposed by all determinists—whether they realise it or not.

II

We have seen what are the consequences of thinking that, on pain of fatalism, we must deny that statements about the future can be either true or false and so deny also that the future will be what it will be and not otherwise, but what are the *reasons* for this ?   That is to say, what are the sources of the belief that logical determinism implies fatalism ?   The answer, I suggest, is that people who believe this are misled by certain confusions about the expressions " it is true now " and " it cannot possibly be otherwise ".

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[1] " Aristotle's Sea-Fight and Three-valued Logic ", *Phil. Review* (1955), p. 272.
[2] " Determinists and Libertarians ", *Mind* (1954), p. 331.
[3] " How I see Philosophy ", *Contemp. Brit. Phil.*, 3rd ser., p. 323.
[4] *Ibid.*

" *It is true now.*"   The confusion about present truth arises in
this way : logical determinists assert not only that every propo-
sition is either true or false, but also that it is true or false, as
the case may be, " once and for all " ; or in other words, that if
a proposition is true it is *always* true and that if it is false it is
always false.   Perhaps it is this expression " always true "
which is the original source of the trouble since it seems reasonable
to infer that if it makes sense to say that something is always
the case it must also make sense to say that it is the case *now* or
at present, and this in turn seems to some people to carry impli-
cations of logical doom or fate.   Thus, in the eyes of the logical
libertarians, the thesis of logical determinism is destroyed by
the following *reductio ad absurdum* :

> If it is the case that for any proposition ' p ', ' p ' must be
> either true or false, and that " ' p ' is true " implies that the
> state of affairs p was, is, or will be the case, while " ' p ' is
> false " implies that not-p was, is, or will be the case, then if
> ' pf ' is a proposition about the future, ' pf ' must be either true
> or false and the assertion " ' pf ' is true " implies that pf will be
> the case while the assertion " ' pf ' is false " implies that not-pf
> will be the case.   Now, this means that something which *now*
> is the case (the truth or falsity of ' pf ') logically requires some-
> thing that is *not yet* the case (either the state of affairs pf or the
> state of affairs not-pf) ; hence, either pf or not-pf is logically
> doomed to occur or not to occur no matter what anyone might
> do to bring about the contrary.   But since this is ridiculous,
> it follows (by denial of the consequent) that it cannot be the
> case that every proposition is either true or false.

The argument is formally valid but the conclusion will be true
only if the following three suppositions are also true :
   (1) that it strictly *makes sense* to talk about statements being
         true or false " now " or " already ",
   (2) that whenever we say that a statement about the future
         is true or false we mean that it is true or false now, and
   (3) that if statements about the future were at present true
         or false, the future would be unalterable by anything
         that anyone did since, as Prior puts it, " what is the
         case already has passed out of the realm of alternative
         possibilities into the realm of what cannot be altered ".[1]
I propose to show that each of these is false.   Of course, since
both (3) and (2) themselves presuppose (1) it would suffice to
show that (1) is false, but, as there is something to be learnt from
a complete diagnosis, we shall consider all three.

[1] " How I see Philosophy ", *Contemp. Brit. Phil.*, 3rd ser., p. 323.

(1) The first supposition betrays a confusion of thought about what is meant by saying that if a statement is true it is ' always true '. Determinists *do* say this but they do not say what the logical libertarians think this implies, *viz.* that it makes strict sense to talk about the truth-value of a particular proposition or statement at a particular time so that, for instance, propositions about the future can significantly be said to be true, or false, at present. The point is that the inference from " true always " to " true now " is illegitimate since the former means " true at *any* time " while the latter is ordinarily taken to mean " true at *this* time but not at some other time "—which is very different. To put it another way : the " now " in the expression " true now " either is to be understood here, as elsewhere, as allowing the possibility that whatever is said to be the case " now " might not be so at some other time, in which case to say that a true proposition (which is true always) is " true now " is to assert a contradiction ; or else its use in this expression is both redundant (if it is true at all it need not be said that it is true now) and seriously misleading (the mere fact that it *is* said suggests that something significant is being added to the claim that it is true, *viz.* that at some other time it might not be true). According to logical determinism, then, there is no answer to the question, " It it true or false *now* that so-and-so was, is, or will be the case ? " since the truth-values of propositions do not admit of temporal distinctions. This is what is meant by " the timelessness of truth " and, for that matter, of falsity too ; that is, it just does not make sense to ask at what particular time a statement is true or false.

Logical libertarians might reply, however, that on this point ordinary usage is on their side. They would remind us that in actual fact we commonly do speak as if the truth or falsity of our statements could alter with the passage of time, *e.g.* when we say such things as, " Yes, admittedly it was true in those days that five pounds a week was considered a good wage, but it isn't now ", or again, " What you are saying about the stability of the Canadian economy may be true now but it won't be in a few years' time ". The objection seems to gain further weight still from Prior's contention that " ancient and medieval usage was generally such that logicians could speak (as Aristotle did speak) of ' Socrates is sitting down ' as a ' proposition ' which is ' true at those times at which he *is* sitting down and false at those times at which he is not ".[1] But we may grant all this without conceding the point at issue, for the libertarian's time-

[1] " How I see Philosophy ", *Contemp. Brit. Phil.*, 3rd ser., p. 322.

dependent truth-values are significantly different from those of ordinary usage, past as well as present, in at least two respects.

In the first place, the so-called 'statements' of which these varying truth-values are predicated are not the same : although it certainly is the case that the truth-values of *grammatical* statements or declarative sentences can be said to vary from time to time, and even from place to place and speaker to speaker —consider, for example, the declarative sentences " It rained yesterday " (said on different days), " We are not far from the sea " (said in different places), and " I am twenty-six years old " (said by different persons)—this does not settle the question at issue which is not whether *grammatical* statements, *i.e.* indicative sentences, have varying truth-values, but whether *logical* statements or *propositions* do. The relevant difference between them is that, on the one hand, a ' statement ' (in the grammarian's sense of the word) does not always specify, or specifies only incompletely, the precise state of affairs to which it refers, *i.e.* it contains words and phrases (*e.g.* " yesterday ", " we ", and " I " in the above examples) whose reference is vague, ambiguous or otherwise indeterminate, so that it admits of being used in different spatial and temporal contexts and by different speakers with consequently varying truth-values, whereas, on the other hand, a proposition or statement in logical form has had these deficiencies remedied by the replacement of vague expressions by precise ones, and further by the use of spatial and temporal indicators specifying the context of utterance, so that its reference is determinate and its truth-value accordingly fixed and unvarying. Thus, as Quine observes : " logical analysis is facilitated by requiring . . . that each statement be true once and for all and false once and for all, independently of time. This can be effected by rendering verbs tenseless and then resorting to explicit chronological descriptions when need arises for distinctions of time." [1] He illustrates this point when, in his discussion of the (grammatical) statement ' There was at least one woman among the survivors ', he writes : " The way to render Mr. Strawson's example is ' ($\exists$x) (x [is] a woman. x was among the survivors) ', with tenseless ' [is] ' and, as always, ' $\exists$ '. The ' was ' here involves reference presumably to some time or occasions implicit in the missing context ; if we suppose it given by some constant ' D ' (*e.g.* ' The sinking of the Lusitania '), then the whole amounts to ' ($\exists$x [is] a woman. x [is] among the survivors of D) ', tenseless throughout." [2] Clearly

[1] *Elementary Logic*, p. 6.
[2] " Mr. Strawson on Logical Theory ", *Mind* (1953), p. 442.

the admitted instability of truth-values in ordinary usage, where
the ' statements ' of which truth and falsity are predicated are
merely sentences in the indicative mood, does nothing to refute
the contention that it does not strictly make sense to admit
temporal distinctions and variations in the truth-values of
propositions, *i.e.* statements in logical form.   Indeed, to take a
simpler example, as soon as the indicative sentence (about the
future), " I shall be Prime Minister of this country in twenty
years' time ", is supplemented in such a way as to make it quite
clear just what logical statement or proposition it is meant
in a particular context to express, *e.g.* " Robert Hawke of
Masson St., Canberra, is [tenseless] Prime Minister of Australia in
1977 ", we no longer wish to say that its truth-value can vary
with the person, place, or time of utterance, but rather that it
must be either true *once and for all* or false *once and for all*—or at
any rate, no-one but a logical libertarian still wants to say this.

This brings us to the second point of difference.   Not only
does ordinary usage fail to corroborate the logical libertarian
thesis that temporal distinctions can significantly be predicated
of the truth-values of statements *in logical form*, but the temporal
variations that it does recognise in the truth-values of *indicative
sentences* are very different from those which the logical libertarian
urges upon us.   Thus, consider the crucial case of an assertion
about the future, *e.g.* (to borrow from Quine),[1] " The Nazis will
annex Bohemia " as uttered before 1939.   According to ordinary
usage, this assertion was true before 1939 but is now false.   But
according to the logical libertarian view neither this assertion nor
the proposition which it expresses, *viz.* " The Nazis annex [tense-
less] Bohemia after 9th May 1939 ", is either true or false before
9th May 1939, although it *becomes* true on that date and remains
so thereafter.

Further difficulties in the view that it makes sense to talk
about propositions being " true now " but " undecided " at some
time in the past are brought out by Waismann.   There are some
philosophers, he tells us, who seem to suppose " that a statement
referring to an event in the future is at present undecided,
neither true nor false, but that when the event happens the
proposition enters into a sort of new state, that of being true.
But how are we to figure the change from ' undecided ' to ' true '?
Is it sudden or gradual ?   At what moment does the statement
' It will rain tomorrow ' begin to be true ?   When the first drop
falls to the ground ?   And supposing that it will not rain, when
will the statement begin to be false ?   Just at the end of the day,

[1] *Elementary Logic*, p. 6.

at 12 p.m. sharp ? Supposing that the event *has* happened, that the statement *is* true, will it remain so for ever and ever ? If so, in what way ? Does it remain uninterruptedly true, at every moment of day and night ? . . . We wouldn't know how to answer these questions ; this is due not to any particular ignorance or stupidity on our part but to the fact that something has gone wrong with the way the words ' true ' and ' false ' are applied here." [1] The fact is, he concludes, that " the clause ' it is true that– ' does not allow of inserting a date. To say of a proposition like ' Diamond is pure carbon ' that it is true on Christmas Eve would be just as poor a joke as to say that it is true in Paris and not in Timbuctoo." [2]

(2) The second supposition can be dismissed summarily by observing that since, as has just been shown, it does not make sense to admit temporal distinctions and variations in the truth-value of *any* propositions, it cannot make sense to admit them concerning propositions about the future. Thus, it should be clear from what has already been said that when we say that a proposition about the future is true we do not mean to say that it is true ' now ' in any sense which implies that perhaps it was not or will not be true at some other time : rather do we mean that, if it is true, it is true *now and always*, and similarly that, if it is false, it is false *now and always*. This fact reacts both to the benefit of logical determinists and to the detriment of their opponents, the logical libertarians, inasmuch as it not only helps to clear the logical determinist of the *reductio ad absurdum* which the logical libertarian presses against him but also, in so doing, deprives the latter of the argument on which much of the plausibility of his own view rested. Thus, if the logical determinist, when he says that all propositions, including those about the future, are either true or false, is not committed to saying that they are *now* either true or false, it no longer sounds so plausible either to say that he is committed to a fated future, or to reason on the basis of this (*i.e.* by denial of the consequent) that since fatalism is unacceptable such propositions must have some *other* truth-value, *i.e.* be neither true nor false but indeterminate.

(3) The third supposition, which involves the unwarranted inference from present truth about the future to a logically doomed future, takes several forms each of which needs to be examined and rejected in turn.

(*a*) For a start, consider the inference in the form in which Prior states it, *viz.* " what is the case already . . . cannot be

[1] " How I See Philosophy ", *Contemp. Brit. Phil.*, 3rd ser., p. 455.
[2] *Ibid.* p. 456.

altered ''. Even if we were to allow the propriety of talking in
this sort of way about propositions being ' true now '—and we
do not—there would be two further objections to make. In the
first place, that which '' is the case already '', in Prior's argument,
is simply the *truth* of ' pf ', *i.e.* of some proposition about a future
state of affairs, *not* that state of affairs itself. To assert the
latter would be to assert a contradiction for if a state of affairs
is '' already the case '' it cannot be a *future* state of affairs. It
should be noted that there is no corresponding tendency to
argue that the truth of an assertion about a past state of affairs
implies that that state of affairs '' is the case at present '', which
would also be a contradiction. The truth of a proposition about
the future implies, not that something *is* the case, but that
something *will* be the case, just as the truth of a proposition
about the past implies, not that something in the past exists *now*
but that something in the past *did* exist—in the past. Secondly,
the expression '' cannot be altered '' troubles us only because,
as it is used here, it is, as it were, swimming out of depth. We
know of course, what it is like to change or alter things contem-
poraneous with our efforts and activities, and even what it is like
to alter our plans for the future, but what would it be like to
alter the future itself ?   In a strict sense, what the future holds
for us cannot be altered for it would then not be part of the
future. But does this mean that the future is fated and that it
owes nothing to our efforts and activities ? We do not think
that the past was fated because it cannot be altered. Why, then,
should we think this of the future ?   Certainly, the future cannot
be changed from what it is to be (isn't this a tautology ?), but
equally certainly it *can* be altered from what it *might* otherwise
have been *if* we had not taken the actions that we did. The
notion of ' altering ' bedevils the real issue which is not whether
the future will be what it is to be but rather whether *what* it is
to be owes anything to our present or future efforts and activities
—and on this question the logical relationship between a true
proposition and what it is true about has no bearing.

(*b*) The fact that something which '' is the case already '',
viz. the truth of some assertion about the future, necessitates
something which is not yet the case, *viz.* the future state of
affairs about which the assertion is made, is sometimes taken to
mean that so-called present truths are the *causes* of what they
truly assert about the future, and this latter is thought to involve
fatalism. Of course, this form of the supposition would not
even arise if it were not for the initial muddle about '' present
truth '', but even apart from this it must be rejected on two

other grounds. The first is that it assimilates logical necessity to causal necessity. It may be natural, but it is nonetheless wrong, to think that whenever something present necessitates something future the necessitation is causal. Thus, in the case in question, what is supposed to be ' present ' is not an event, which *is* the sort of thing that can be a cause, but the truth of a proposition, which is *not*. No doubt it makes sense to say that someone's *assertion* (which is an event) about the future is the cause of the future event which it is true about, *e.g.* a prediction about the effects of making that prediction, but this does not affect the point at issue. The relationship between so-called " present truths " and what they are true about is that of logical necessity not causal necessity ; that is to say, it would be a contradiction, not just a factual error, to say that the one was not " followed " by the other, and this, of course, simply confirms the fact that the relationship between truths and what they assert to be true is a non-temporal one. The second mistake is to assimilate causal necessity to fatalism. It should suffice to observe that there is a vast difference, indeed an inconsistency, between saying that the future will be what it will be solely because of what the present is (causal determinism), and saying that *what* the future will be owes nothing to what our actions at present are (fatalism).

(c) There is a further argument still which pretends to derive the fatalist conclusion from the supposition that statements about the future can be true at present. It proceeds in two stages : it is contended, first that propositions about the future could not be either true or false unless the future states of affairs which they are about were causally determined by antecedent states of affairs, and secondly that to say that the future is caused is to say that it is fated. But the inference fails at both stages ; the second because, as I have just said and therefore need not repeat at length, it erroneously assimilates causation to fatalism, and the first because it confuses causal determination with logical determinateness. Now to say that the future is logically determinate, *i.e.* that it will be what it is going to be, is not to say anything about whether it is also causally determined, *i.e.* whether it will be what it is going to be *because* of what happens before it. It may be but it may not be so determined. The inference from ' x is causally determined ' to ' x is logically determinate ' is a legitimate one but the reverse is not. Thus when we say that statements about the future are either true or false we are saying only that the future is logically determinate and this, by itself, does not warrant the further inference, on which the above argument depends, that it is causally determined.

It might be suggested, however, that we could never know that any given statement about the future was true or false unless the future were causally related to the present, *i.e.* unless there were causal laws, a knowledge of which along with a knowledge of initial conditions would enable us to make correct predictions about the future. But this, of course, is quite beside the point, for the issue is not whether propositions about the future can be known to be true or known to be false but whether they can *be* true or *be* false. Moreover, predictive inference proceeding by way of our knowledge of causal laws, is not the only logically qualified candidate for the job of telling us what the future will be, for although the empirical evidence for the occurrence of precognition may be, as it undoubtedly is, still very weak, there are no fatal objections to it on purely logical grounds. Now all that is required for precognition to be possible is that the future which is to be precognised should be logically determinate—not that it should be causally determined. Thus, it is not hard to conceive of a world in which all events occurred purely at random, or without causes, and in which a being with the power of precognition could at any moment foretell what was to happen next : indeed, according to some theologians, this is what our own world is like, except, perhaps, that God, the precognising being, is not usually regarded as being in the world but rather as transcending it. Now the mere fact that such non-inferential knowledge of the future is logically possible means that we can conceivably know whether our statements about it are true or false even although it is not causally related to the present. That is to say, all that precognition demands is that there should be a determinate, not a causally determined, future to be precognised.

" *It cannot possibly be otherwise.*" It seems that many philosophical confusions arise from the failure to distinguish between the multiple senses of the correlative terms ' necessary ' and ' impossible '. One of these confusions is that of thinking that if it is logically necessary that the future should be what it is going to be, *i.e.* that it is logically impossible for it not to be what it is going to be, there can be no point in doing anything since whatever we do must have happened anyway, and whatever we do not do could not have happened even if we had tried to make it happen. In other words, the supposition that the future cannot possibly be otherwise seems to imply that the future is fated and that all our efforts are doomed to frustration.

Now I suggest that we need only remind ourselves of the distinction between logical necessity and impossibility, on the

one hand, and coercive necessity and impossibility, on the other hand, in order to see that this inference is completely unwarranted. What follows from the fact that a proposition about the future, e.g. " E will occur at tf ", is true is simply that E *will* occur at tf, not that E will occur at tf in spite of anything that we may do to prevent it.   Thus, if E-at-tf is something that I am going to do, then it follows only that I *will* do E at tf, not that I shall be *forced* to do it irrespective of what else I want and try to do. Certainly, whatever we are going to do must (logically) be going to happen —otherwise it could not be said that we are going to do it—but it is seriously misleading to say that it must happen " anyway ", *i.e.* as if what is going to happen owes nothing at all to my efforts and activities.   No doubt there will be some events in the future whose occurrence will owe nothing whatever to my efforts to bring them about just as there will be some whose occurrence will thwart my efforts to prevent them, but this is no more surprising or ominous than is the fact that in the past there have been events which I did not produce and events which I could not prevent.   There is no more reason for saying that the future is fated because it will be what it will be than there is for saying that the past is fated because it was what it was or the present because it is what it is.   We do not say the latter, so why should we say the former ?   It is clear that the sense in which things " cannot possibly be otherwise " because they are what they are is very different from the sense in which they " cannot possibly be otherwise " because other things *compel* them to be what they are :   in the former sense *nothing* can possibly be otherwise, while in the latter sense at least *some* things obviously can.   It is further important to realise that the future, of which it is asserted that it is logically necessary that it should be what it is going to be and logically impossible that it should be anything other than it is going to be, includes whatever I am going to do and the consequences of what I am going to do. This means that the same logical necessity which requires that the future should be what it will be, also requires that such efforts as I might then make should have the effects that they are going to have ;   that is to say, it does not make my activities futile since it extends its logical guarantee to all events—my actions and their consequences, if they have any, included. Similarly, if it is logically impossible for the future to be different from what it will be, it is also logically impossible for the causal efficacy of my future actions to be other than it is going to be : if they are to be frustrated (and this often happens without our thinking that they are *doomed* to be frustrated) then certainly it

is logically impossible that they should not be frustrated, but equally if they are to be effective it is logically impossible that they should not be effective.   The answer to the factual question which *will* be the case, *i.e.* whether my activities will make any difference to the future or whether they will not, obviously cannot be deduced from the logical truism that both they and the future will be what they will be whichever is the case.

Thus the final and decisive argument in defence of logical determinism against the bogey of fatalism is that you cannot derive fatalism from a mere tautology or set of tautologies. Fatalism pretends to give us definite and indeed quite startling information about the world we live in—information which, if it were true, would enable us to distinguish this world from other possible worlds : it asserts that in our world the present is causally discontinuous with the future and so concludes that our actions are completely futile since nothing we do can influence the future.   But logical determinism, since it asserts no more than a set of tautologies, tells us *nothing*—that is, it tells us nothing that would enable us to distinguish this world from any other simply because it tells us what this world *and* every other possible world *must* be like, *viz.* that its future (if it has one) will be what it will be and logically cannot be otherwise. Whereas fatalism makes a factual assertion that this or that is the case, logical determinism asserts only the logical truism that *whatever* is the case is the case, and there clearly can be no passage from the latter to the former.   As soon as we understand this, fatalism loses its terror, logical determinism stands vindicated, and the case for logical libertarianism collapses for want of an argument.

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V.—PRIMARY QUALITIES IN PHYSICAL EXPLANATION

By Fanchon Fröhlich

" These insensible corpuscles being the active parts of matter
and the great instruments of nature, on which depend not only
all their secondary qualities but also most of their natural
operations ; our want of precise, distinct ideas of their primary
qualities keeps us in an incurable ignorance of what we desire to
know about them. I doubt not but if we could discover the
figure, size, texture and motion of the minute constituent parts of
any two bodies, we should know, without trial, several of their
operations one upon another. . . . The dissolving of silver in
aqua fortis and of gold in aqua regia, and not vice versa, would
then perhaps be no more difficult to know than it is to a smith to
understand why the turning of one key will open a lock and not
the turning of another."

Locke, by maintaining that it is specifically through the group
of primary qualities that bodies act upon each other, reflects in a
somewhat distorted way a general feeling that people have for
what constitutes an ultimately satisfying physical explanation of
interactions among material objects. Thus when someone
observes wheels turned by cogs of various colours and materials,
having in common only a certain shape and a fairly high degree
of rigidity, he will be satisfied with an explanation of how they
do it which tells him how the teeth in the cog fit into holes in the
wheel and, being rigid or strong, force it to turn when the cog
turns ; or when he sees a great variety of objects of all shapes,
colours and smells pushing others aside or breaking dishes on
which they fall, he will understand how they do so if told that
they are all hard, heavy, and have great force or momentum.   If,
on the other hand, he notices that many different types of objects
which appear to have in common only a peculiar smell, always
break or knock out of their way whatever they hit, or go up in a
yellow flame when dropped into a certain kind of acid, he will not
accept their smell as an explanation of what they do, nor believe
that they do it by virtue of their smell.   In the first case he will
use the smell as a clue to identify the common material of which
all the objects are made.   He will then, perhaps, recognise that
this material is one of the heaviest or hardest substances and use
its weight or hardness to account for the operations of the objects
made of it.   The second case is more complicated.   It would be
explained only within the framework of a theory whose terms
were very different from smells although not necessarily like those

offered above. Even then, although one could explain why the
reaction took place and why it was as intense as it was, one could
not explain the yellowness of the flame. This is only correlated
with certain physical properties, and no attempt to explain the
correlation—why this particular colour is always associated with
certain physical occurrences—would be made.

These examples suggest that one does not discover the property
by reason of which a certain operation takes place or in terms of
which a satisfactory explanation of the operation can be given
merely by carefully and systematically observing in all such
operations whether the property is always present or whether in
the absence of the property the operation itself is interrupted.

Locke, however, appears to have much more specific knowledge
of those properties by which bodies operate on each other than
have most people, for he would exclude three of the acceptable
reasons used above—momentum, weight and hardness, the last
being secondary in contrast to solidity which is primary. He
could not have acquired this knowledge from studying his
contemporary science, for his primary properties are not those of
mechanics. Besides, he admits that he cannot comprehend
gravitational attraction, nor therefore weight, and he shows no
tendency to include weight among the primary qualities merely
because it was basic to Newtonian physics. Mass and force are
fundamental concepts in mechanics, and elasticity is assumed,
but none of these are in Locke's list of primary qualities, whereas
impenetrability, which is for him primary, is too weak a quality
to have any physical effectiveness. It might well be that,
whenever he thought of the world-picture given by science, he
had in mind not the equations or propositional statements about
how things happen but a picture of the billiard ball model of
mechanics, or even more likely, since he mentions this, of the
mechanical operation of a watch. Here size and shape are very
important ; one can *see* how a cog turns a wheel by fitting
together with it. The rigidity of the parts, which would be of
interest to Newtonian mechanics, is taken for granted in such an
operation ; it is the directly visible relations that seem to be
important. Yet what actually turns the cog is the gradual
release of tension in a spring. One would perhaps say most
accurately that it was by means of this tension that a watch ran.
However, Locke excludes tension from his list of primary
qualities while including absolute impenetrability. Yet this is
such a physically weak concept that it cannot distinguish between
water and steel, both of which are absolutely impenetrable, and
is therefore incapable of indicating why an operation has taken

place in one way rather than another.   In fact, it is difficult to find a source from which Locke could consistently have drawn his group of qualities by which bodies act upon each other. Perhaps there is none.   It seems rather that he has assumed that those properties which he found necessary to the notion of matter must be those which would ultimately account for all actions of one body on another, without noticing what a great and gratuitous assumption this was.   Thus he thought he could present the general notions of physics after a purely logical investigation.

Yet his thesis that there is a separable group of properties " from which flow all powers in bodies " contains an important insight.   To put it with reference to our possible knowledge of objects, there is a group of properties in terms of which we naturally couch our explanations of how bodies act upon each other.   Locke recognised that this group is not given merely by repeated observation of how objects do act on each other, but rather provides us with a standard for our understanding of such actions.   But he neither gave the most compelling reason for isolating such a group, nor did he include in his group all and only those properties in terms of which explanations of physical interactions are most satisfactorily given.

I am now going to try to show how a particular group of properties of objects is implicitly selected for use in ultimate physical explanations and how such a usage is justified.   This way seems more plausible than Locke's both because it has its source in those situations in which we say that we do fully understand how some change in an object has come about and because it yields properties which are actually used in some scientific explanations.

One can describe very fully how one object has acted on another if one has watched all of the transaction, and yet, after such a description, one might still say : " Although I have told precisely the way it happened, I can't understand how the first state of affairs could have resulted in the last."   For instance, a small soft piece of grey material might have lain on a table peacefully with no sign of activity in it and then been dropped into a jar of cold water.   There might then have been an explosion, fire and hissing, and the bit of material might have skidded across the water heating it to boiling point, and finally dissolved leaving, instead of water, a powerful acid and a cloud of gas in the room.   Just to recount this sequence of events is no explanation.   Such phenomena have, in fact, been explained in terms of ions, i.e. atoms with an excess or deficiency of electrons, grasping each other to form a new molecule, accompanied by the release of much force.

The picture usually given in elementary chemistry books is that one atom grasps or attaches itself to another thus forming a valence bond by fitting neatly into it.   Now why is this a satisfactory explanation, and what has it in common with the examples already given ?   All these explanations rely ultimately, I believe, on the understanding of how one might have done something himself, with his own hands.   Perhaps, however, this is not yet the full description of a case in which one knows exactly how something came about.   One can imagine noticing a strange doodle on the edge of a paper and asking " How did that get there ? ", and receiving the reply from the writer, " I have no idea.   I suppose I just did it unintentionally while I was thinking about something else."   So it is rather in those situations in which we know what we are doing, intend to do it, and do it, that we say we understand completely how it has happened.   It is logically involved in " doing something voluntarily " or " intending to do it " that we know what we do and how to do it. A step beyond this—" How did you move your hands ? "—is illegitimately metaphysical.

Now if we find on what sort of occasions we interfere in the world, we can observe in what circumstances we can be said to understand an occurrence fully.   At first glance it seems that the variety of things we can do in the world is almost unlimited—we build monuments, make music, cause explosions, paint pictures, tie knots, etc.—but on reflection it becomes obvious that what *we* actually do in all of these cases is to move objects.   We pile stones on top of each other and put cement between them, we pull and push bows across strings, we put chemicals or paints together, we move ropes around in a special way.   We can heat a room, but only by bringing in logs, by rubbing a match across something rough and holding it to them or by a more modern and complicated process working on the same principle.   The different effects that we are able to achieve depend on the various things we move and the ways in which we move them.   We would seek to explain apparent exceptions to this on the same model.   Thus the Yogi's ability to raise his own temperature voluntarily might be explained by his having muscular control over his own vascular system.   The occasions, then, on which we can be said to have completely understood some change that has taken place are those on which we have intentionally moved some objects.

Now if we list those factors which are necessarily involved when we move objects, we should have a roughly accurate selection of those properties in terms of which we can understand the operations of bodies on each other or by reason of which we say objects

act on each other. In all such situations we exert force against a
body which, if stationary, resists our efforts to move it, or, if in
motion, resists our efforts to stop it. A fair degree of hardness or
rigidity of some of the objects taking part is necessary for such
operations and is involved in resistance ; we cannot push or pull a
liquid, but we can use our hands as a container for it. In those
movings which are accomplished by hitting rather than by
pulling or dragging, the degree of elasticity of the object is
important in determining whether it bounces back or stops dead.
The weight of the object determines how much force must be
exerted on our hand if we try to prevent it from falling. Shape is
important in grasping an object and in determining whether it can
be freely rolled or must be shoved and into what places it will fit.
Size is not so directly important in moving an object, since the
amount of force we must exert on a large, light body need be no
greater than that necessary to move a small heavy body. How-
ever, size determines where we can deposit the object—what
space it will fit into.

There are, however, various other factors which would be
included in a complete description of any situation of doing
something oneself, which could in no way be said to be those by
which the operation took place and which are not employed in
an explanation of it. Such would be exhaustion or pain, all
feelings of satisfaction or annoyance, and other things one was
thinking of at the time, the colour of one's clothes, the expression
on one's face or the colour, smell or emotional overtones of the
object. A differentiation of all these from the earlier mentioned
elements must be involved in any situation of learning how to do
something. Thus a child learns at an early stage that, however
much he wants his ball and contorts his face, or whatever the
colour and smell of his ball, the ball will not come to him unless he
reaches out and grasps it. This is a sort of learning, based on
experience of how one succeeds in making changes in the world,
which is more primitive than that based on the observation of how
objects act on each other. One might say that it was a learning
" how ", rather than a learning " that ". Therefore, by the time
one has learned to do anything for himself, one will have a fairly
good idea about which elements in a situation in which one is
doing something are responsible for its accomplishment. In
order to do it intentionally one must know.

But now how is it that the operative elements in such a
situation of doing something intentionally oneself can be regarded
as properties of inanimate bodies ? How can such elements in
our manual operations be observed at all in material objects ?

Is there anything really in common between my exerting a force
on something and an object's exerting a force, except that we
describe both situations in the same words ? These questions
will be answered by showing how our methods of interfering in
the world and of observing these properties coalesce. Thus it
will be shown how one of the basic ways of observing the
momentum, force, weight, motion and shape of a body is by oneself
acting on the objects in which one wants to observe these
properties, namely by oneself moving or by permitting one's own
body to be moved.

We do not observe these properties merely by perceiving
them, as we observe such properties as red and sweet-smelling,
nor only by noticing the habitual behaviour of an object, as we do
in observing that something is soluble. We can directly see that
a body is square and large and moving, and we can see that it is
hard, heavy, elastic and has great momentum by watching how
it moves and moves others. Moreover, we can also observe these
properties by touching or feeling the objects. But the way in
which one feels the hardness, force, heaviness or motion of an
object is quite different from the way in which, for instance, one
smells the sweetness or sees the colour of a rose. For the latter
two do not involve any direct interaction with the object observed,
whereas the perception of all of the qualities sensed by touch
does. Thus one can feel that something is hard in various ways.
The feeling of hardness is not a single type of " sensation ", as the
feeling of coldness is. For example, feeling that an object is hard
might consist in muscle strain plus a slight finger-tip feeling when
one is squeezing it, or it might consist in pain and the kinaesthetic
sense that one's hand was not moving plus the feeling of how much
force one was exerting on the object. Any such way of feeling
hardness always includes one's own body doing something—
squeezing or recoiling. All of these terms—" muscle strain ",
" feeling of exerting force ", " kinaesthetic sense "—involve a
reference to interactions between one's own body and objects.
We have no vocabulary for such feelings, as qualitatively distinct,
as we do for different colours. They can be differentiated in
language only in so far as they are distinguished by reference to
their source. In order to describe or indicate them one must say
how one's own body is acting and being acted on by other material
objects.

One can notice that something has force or momentum by
watching what it does ; it knocks other things out of its way,
makes indentations in things, or shatters obstacles in its path, etc.
But the only method of directly feeling its force or momentum is to

stand in its path and to feel oneself either being pushed aside or stopping it.   Also the only way to feel the heaviness of something is by supporting it or lifting it oneself.   The feeling of motion has similar features, although in this case one need not act *on* the object, but one must move oneself or let one's body be moved. There are two distinct ways of feeling motion.   One feels the acceleration of one's own body when the train starts, when the boat rolls in a high sea, when the car goes around a corner, etc., by means of an organ hidden in the occipital lobes.   Since the discovery of such an organ we tend to be more willing to call this a sense.   This might, however, be called a " private sense ", in as much as its domain is limited to one's own body.   In order to feel the movement of something else, one must move with it oneself or let one's hand, say, follow its path.   It might be maintained that the observer does not really feel the other thing moving ; he feels it at rest in his hand or under his fingers, and he feels himself or his arm moving along with it.   Feeling his arm being moved involves the second way of feeling motion.   It is not by the special organ in the occipital lobes that one can feel that one's arm is moved from left to right in following the motion of something, but rather by what might be called a complicated sort of muscle sense which makes one aware in an unobtrusive way of the relation of the parts of one's body to each other.

" Muscle sense " is rather obscure, and language analysis is not very helpful here because one does not have much occasion to talk about what is always present to oneself and not observable by others.   Therefore, there are given no fixed ways of describing such feelings as that of the muscle sense of relative positions of parts of the body.   Yet this whole " body's feeling of itself "—of its position, the pressure it is exerting, how its possible motion is hindered by other objects, etc.—is of great philosophical importance.   Whereas one might imagine oneself blind or deaf or unable to smell or taste, it seems difficult to conceive that these internal and touching sensations should cease.   Then there would be no ground for distinguishing one's own body from the rest of the world except that one observed it to move when one wanted it to move.   Hence a Cartesian split between extended, non-feeling matter and sensing, feeling and thinking mind would seem somewhat more plausible.

To continue with the examples, one can feel the elasticity of a ball in the pressure it exerts on the fingers when it is squeezed. Feeling the shape of something that is larger than the hand is similar.   One can feel the pattern that one's hand describes in passing over the surface of the object, exerting a constant

Is there anything really in common between my exerting a force
on something and an object's exerting a force, except that we
describe both situations in the same words ? These questions
will be answered by showing how our methods of interfering in
the world and of observing these properties coalesce. Thus it
will be shown how one of the basic ways of observing the
momentum, force, weight, motion and shape of a body is by oneself
acting on the objects in which one wants to observe these
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To continue with the examples, one can feel the elasticity of a ball in the pressure it exerts on the fingers when it is squeezed. Feeling the shape of something that is larger than the hand is similar. One can feel the pattern that one's hand describes in passing over the surface of the object, exerting a constant

slight pressure against the object in order to determine where the surface resistance begins and ends. Thus in order to feel directly weight, force or hardness as properties of objects, one has to interfere with the objects in a pushing and moving way similar to that by which one can make changes in them. In order to feel, as distinct from seeing, their motion and shape one has to move one's own body. The way of observing these properties is thus similar to the basic way of doing anything physical in the world. The difference in the situations consists in the different purposes for which one pushes or moves things.

The above sketch offers an answer to the question of how, without long and systematic observation, people seem to know by what properties bodies can act on one another. In other words, it points out a possible basis for people's general willingness to give an explanation of why bodies act upon each other as they do, in terms of a certain fairly definite selection of their properties. This attempt to show why a certain set of properties is preferred as most suitable for physical explanations seems more fruitful than do those attempts whose source I have been able to find in Locke. It suggests why mechanical explanations seem so intrinsically plausible, and it leads, moreover, to properties which are actually important in physical laws and theories.

Yet some of the fundamental properties of planetary mechanics and the structure of matter, even for seventeenth-century science, did not fit into this scheme, e.g. attraction at a distance and the cohesion of the particles of matter. We cannot move objects at a distance, nor, without some discernible glue, do they stick to and follow us merely on contact. They have to be grasped and pulled. Many scientists and philosophers rejected gravitation as a fundamental property of matter and even rejected the whole Newtonian system of explanation, on the ground that it was inconceivable that one body should act on another at a distance. This rejection was not due to their never having seen material objects attract each other at a distance, for they were familiar with magnets ; it was due more probably to their keeping, as an implicit model of how bodies can act on each other, our ways of voluntarily acting on objects. As long as one kept such a model of explanation, even though many or all objects moved each other like magnets, namely, by attracting or repelling each other more or less strongly, such interactions would seem mysterious and " inconceivable ". Modern physics says that fundamental particles do attract and repel each other at a distance. The properties by which its entities act on each other are not those that are effective in manual operations. Since attraction and repul-

sion, as distinct from pushing and pulling, are the terms used by those who are most concerned with investigating how bodies act on each other, it seems plausible to conclude that manual operations provide too narrow a model into which to fit all interactions among objects. As elsewhere, carrying a model much beyond the facts which have originally suggested it misleads one into thinking that one has gained necessary, but factual, general information about how things in the world behave.

Yet, in a primitive way, in order to understand an explanation fully, it does seem necessary to have a model in our own experience for its terms. A range of human activity wider than that of manual operations, including one's feelings and emotions, provides a model for the principal terms of modern physics, "attraction" and "repulsion". One feels oneself, in an emotional way, attracted to and repelled by various objects and persons whom one loves or hates. The analogy of a state of emotional tension might enable one to understand what it is like for a physical entity to have a tendency to move in a certain direction, even when it is actually kept at rest by other factors. The French word for magnet is "aimant". Such a model is implicit in the physical explanations of Aristotle and the Mediaevals, when they say that stones fall because they seek their natural places and that, in general, bodies want to be in their natural places. It is somewhat more explicit in the appetition of Leibniz's monads. It was very evident and picturesque in the erotic terms in which alchemists described chemical affinities, and the love and hatred that they ascribed to the elements are echoed in deanthropomorphized form in the attraction and repulsion of atoms in chemistry. The fundamental entities of modern physics act on each other chiefly by repelling and attracting. Such a model can be richly suggestive in one's thinking and an aid in explaining and teaching. But, of course, one cannot deduce from it anything about how bodies will or can move and act upon each other. New discoveries might force scientists to abandon it in favour of another, or, if they were so unfortunate as to be unable to find a different model to fit the equations and terms of their new theories, by none at all. No model can be said to be necessary. Locke would object to this model, although it might fit and function excellently, on the ground that it provided a merely mental analogy for what bodies do, and that this must be inappropriate. Yet this seems a serious objection only when stated in the context of a philosophical two-substance theory. Such a theory, however, would not provide a very sound basis for rejecting any aid to understanding.

VI.—WORSHIP, SUPERLATIVES AND CONCEPT CONFUSION

By S. Coval

MANY concepts appear as they are not. Foremost among them
has persisted the visage of the concept of God. Part of the
reason for this endurance is that analyses of ' God ' have long
been shunted aside with such disclaimers as " but the truly
inexpressible . . . ", " paradox and truth . . .", " the inadequacy
of language . . .", " non-ordinary language . . .", " uniqueness
of religious categories . . . ", " new rules . . .", " like poetry . . ."
and so on.

I do not propose here to go frontally into the cogency of these
disclaimers, to ask, for instance, directly about the position of
those grand and comparatively renewed categories of ' the
ineffable ' and ' the logically new ' which might include metaphor,
paradox, trope, symbol, etc.

I do, however, propose to indicate what I think are some new
yet nodal points in the logic of the concept of God, points which
show in turn that religious discourse as well as ordinary language
have been infused with uses which involve category confusion.
And finally, I think, this will then have something to say,
though perhaps obliquely, about the theologians' traditional
stronghold of the unutterable.

(1) *There is an analogy or continuum between some approbative
expressions and some religious expressions.*

Whatever else is still the case in philosophy there is no longer
much resistance to the point that ' good ' can and does occur
sometimes merely as an approbative expression. ' Like ',
' approve ', ' fancy ', ' desire ', ' prefer ', ' choose ', ' endorse '
are its cognates. It is hardly likely to be false, for instance,
that what you select as good you will also approve of, or perhaps
like or desire. But because ' good ' is the most general word of
approval in the English language it will apply sometimes where
' like ' and ' desire ' do not. This, though, and the performatory,
persuasive or pragmatic functions of these words, do not figure
here.

You find something good, you like it, you approve of it. There
is a degree of commitment or acceptance or for-ness in that.
What then if you love, cherish, idealize, honour, respect or esteem ?
Whatever else is the case your degree of commitment has in-
tensified. Notice, for instance, that " acceptance " would now

not serve to name this degree.   Thus, as it is hardly likely to be false that what you think good you in some sense approve of, so it becomes something of a joke to ask :   I know you love, honour, cherish him utterly, but do you care for him ?

In other words " I love him but don't approve of him " is a paradox unless it is taken to mean " . . . don't approve of him in this or these regard(s) ".   What would it mean though to say " I love him but don't approve of him in any regard whatsoever " ?

Suppose now in the religious sense you idolize, venerate, adore, revere, glorify, exalt, worship someone or something.   Here it is silliness to pose :   " You worship . . . but you do approve . . . ? " (You aced him but did you win the point ?)

To approve, to love, to worship are all to be for in some fashion or other.   But to approve is to be less so than to love and to worship is to be far more so than either.

(2) *All approval words take an object.   That is why " I worship God " is a redundancy.*[1]

Another rather obvious logical fact is that we may legitimately ask " of what ? " or " for whom ? " of your expressed approval, love or worship.   Can you just worship or love or approve, period ?   That would be like literally seeing nothing.   The expressions in question here are, as some are wont to call them, intentional expressions.   Grammatically they are transitive.

Now if you do have a God you would not be supplying additional information should you add that you worship.   To have a God means that you worship and vice versa.   It should be remembered that what is important here is not this or that method of worshipping.   Perhaps at this point it would be better to use " revere " than " worship " for we are concerned here with the attitude involved and not in this or that ritual.

If you are asked then " What or whom do you worship ? " it is acceptable to answer " Manitou " or " Water " or " the Golden Calf " or " the God of my fathers ".   But you offer nothing when you answer, say to an absolute stranger, " I worship God ".   And of course you will capitalize.   This lack of information occurs not because there are many Gods, so that your answer is troublesome because of its ambiguity as the answer " Athens " might be to the question " Where do you live ? "   It is rather that you have uttered a mere repetition :   " I worship God " amounts to " I worship what I worship ".

The taking of ' God ' then as an object word alongside other

[1] I owe this point, I think, to Jason Xenakis.   Perhaps, though, he is unwilling to claim any responsibility for what has happened to it.

object words is part of the error that has infected ordinary language. We think it an act of homogeneity to answer " Whom or What do you in your religion worship ? " with " Ancestors ", " Children to the Age of 13 ", " Masonry ", " Mountains ", " Cattle ", " Thunderbirds ", " God ". None of the replies except the last, though, would as we have seen leave us completely uninformed. " Whom do you worship ? " remains unanswered by " God ", " a god " or even " The God " except that they verify the question-begged fact that you do worship and that the last reply indicates your attitude toward singularity.

To put this not quite another way : That you worship children until they are the age of 13, or worship ancestors, or thunderbirds, or mountains may or may not be the case, but that you worship your God can never be false.

(3) *If ' God ' in its religious sense means " what one worships " then knowing you I can foretell the nature of your God.* But this is no more a feat than promising to be able to tell you what you take to be good if you tell me what you like, desire, approve of.

If I know to which characteristics you are absolutely, extremely, superlatively, perhaps unremittingly or rabidly or unrelentingly, committed I shall know what must be the nature of your object of worship, of your God. And I shall know this because you cannot be neutral toward or merely approve or desire anything less than the superlative for your object of worship. And that is so because worship is just that superlative-requiring sort of affair. All of this is tautologous. Can you worship a finally imperfect or defective thing ? You cannot, for to have an idea of God is to have as Anselm saw an idea of perfection. Our Gods are absolutely the best we can do. Otherwise they could not be worthy of our absolute commitment, our worship.

We might here remind ourselves of the nature of the prefixes adorning deity descriptions. God is omni-, pan-, trans-. And where being the best of a category would ultimately diminish Him, He must be above and without it : *via negativa.* God is extra-, super-, a-, non-, in-, un-.

It is because of the connection between " superlative ', ' worship ' and ' God ' that one can deduce what must be the properties of a God if one knows the characteristic superlatives of that God-committed individual or culture.

(4) *God and existence.* Something of a case in point to the above is the role the concept of existence plays in deity talk. We might, I think, with much the same general result examine the use of any other superlative deity-description such as omnipotent or omniscient or atemporal or ineffable.

Suppose you worship, you have a God. What are you then willing to say or admit regarding that God and His existence ? Suppose you are willing to have your God denied the category of Being. He cannot be said, in any sense, " to be ". Then, because you do still speak of him, think of Him, believe in Him, He may not merely be said to " not be ". There is a more precise name now for His status and it is indicated by such words as ' imagined ', ' mythical ', ' fictional ', ' figmental '. But then a queer thing has happened to your worshipping. For you now must be willing to say that you worship a Non-Existent, a Non-Subsistent, a Fiction, a Myth, a Something Only Imagined. Can you place your fullest trust, your utmost religious faith, be ultimately and superlatively committed to a Fiction.

Now, within the limits which I am trying to set, one may worship what one chooses. But you are a fool or perhaps a god if you believe you can worship and yet worship a fiction. Unless they are themselves classed as fictional or your worship too is fictional, none of the remaining superlative deity-descriptions can apply to God who isn't. Nor can He help, succour, love, create, punish, have moralized. If He is not real then He can operate neither within nor " above " the real ; unless " operate " too is taken in a fictional sense. You may, though, apply to Him all possible non-fictional superlatives which apply to fiction. He may be most superbly conceived or utmostly imagined or even omnifictional.

The point of all this is to show that " I worship a fiction " or " My God does not, in any sense, exist ", are expressions which are self-destructive and that they are so since ' what I worship ' or ' God ' is a concept to which ' Being ' is essential. To subtract ' Being ' from the description of what you worship is to destroy it as an object of worship ; and at least part of this logical destruction occurs because the subduction of ' Being ' takes with it all other possible deity-worthy descriptions.

This again is likely why both Anselm and Descartes had seen it necessary to hold that the conception of God must include the idea of existence and, more important, why their argument was even to themselves so persuasive. If you have a God, he must exist. That is a tautology.

The trick, though, is to be able to see that existence is necessarily part of God's make-up because it is dictated by the logic of ' worship '. (1) We do not worship Him because he exists or has such and such properties ; rather, " to worship Him " means at least partly that " He has such and such superlative qualifications ". (2) Similarly it is not the case that you like so and so

because you think it is good ; rather, " you like something "
means at least partly that " you think it is good ". Unlike
ordinary ' because ' statements these expressions (1 and 2) cannot
be false : they too are more like repetitions.

Here we have, I think, the crux of God-talk, seeing it, that is,
under the aspect of the extremely approbative. When we see
it as such we understand that descriptions of God are dictated
by the necessity to create omniperfection, to vest our object(s)
of worship with that superlativity worthy of our extreme appro-
bation, our reverence. It is for this reason that God exists.

But then it becomes entirely strange to conjecture about God's
existence, just as it would be entirely strange to conjecture
honestly about the marital status of someone you know to be an
honest-to-goodness bachelor.

Thus questions such as " Does God exist ? " or " Is there a
God ? " are to be seen as : " Do you worship ? " or " Are there
worshippers ? "

(5) *God and ineffability.* I might add a short note to anticipate
a venerable objection certain to emerge from some quarters and
consonant with a promise made at the beginning of this paper.

" It is rather silly to attempt to approach God with logic. He
has a logic of His own—in your sense of ' logic ' He is superlogical.
Furthermore your attempts at understanding His nature, your
method, the difficulties which you see and the rather historic
flagrancies at which you arrive are all emergent from language.
How can we expect the nature of God to come off at all properly
at the hands of this mundane device concocted by our limited
selves for a limited world ? God is above and beyond language
and intelligence. Ineffable."

But this is merely one of the forms of argument we might receive
from any apologist who was having difficulty with his superlatives.
And the matter of God is the matter of superlatives. The rule
for the apologist is : When the superlativity of God is endangered
simply extricate Him entirely from the danger-area or make
Him the utmost in that area. Thus His ineffability, omni-
actuality and so on. Much theology has been an excellent exercise
in one-upmanship.

Washington University, St. Louis

VII.—WITTGENSTEIN AND THE EGOCENTRIC PREDICAMENT

By Carl Wellman

There is a certain picture of human knowledge which is, at least in broad outline, held by many. Our knowledge of the world is composed of a vast number of different judgments. These judgments are not logically independent, but they support one another by a network of evidential relationships. These relations of ground to consequence may be traced backwards to more and more fundamental grounds until one reaches experience itself. Thus the entire system of factual judgments rests finally upon the given. Not only is experience the ultimate evidence for our beliefs, but it is also the source of all our meanings. Most of our words can be defined in terms of more simple concepts, but the most basic concepts are verbally indefinable and can be given meaning only by ostensive definition. Thus language derives its significance from its connection with the given. Each experience is given, however, to a single person. No experience can exist which does not belong to some self, and no two selves can have the same experience. The ultimacy of experience is matched by its privacy.

This epistemological picture seems helpful, but it gives rise to several philosophical problems. If all factual knowledge is grounded in experience, how can one know any reality which is independent of experience ? If experience is private, how can one person know anything about the experience of another person ? If words derive their meaning from private experience, how can one person explain to another what he means ? The picture seems to imply that I can know only my own experience and that I can understand only my own words. This is the egocentric predicament.

Ludwig Wittgenstein has tried to show that this predicament is unreal by showing that the epistemological picture on which it is based is nonsense. My purpose here is to explain and criticize the considerations which led him to reject this picture of our knowledge as misleading. I have taken the liberty of collecting the pertinent remarks scattered throughout his *Philosophical Investigations* [1] into nine groups.

[1] Ludwig Wittgenstein, *Philosophical Investigations* (Oxford, 1953). All references will be to numbered section of Part I rather than to page.

(1) If there were a language whose words referred to private sensations, it would be quite unlike our ordinary language ; for no one could show any other person what he meant (243). In attempting to picture such a language, however, one includes features which could belong only to our public language—a public language being one which is capable of being spoken and understood by more than one person and of being used for communication among them, while a private language is one which could be used by only one person and which could not be taught to others. One thinks of the word " blue " as meaning both the colour of the private impression of the sky and the colour of the sky we all see, but these are very different kinds of meaning (277). Although one says that sensations are not physical objects in the public world, one conceives of them as like membranes which can be peeled off the objects (276). One thinks of experiences as belonging to selves, but ownership can exist only in the objective world (398). The fact that this picture of the language of sensations tends to assimilate it to the very different language of physical objects shows that the basic conception is confused.

I must agree that this picture of the language of sensations is confused. No adequate theory of the meaning relation between words and experiences has yet been formulated. The nature of experience itself remains in doubt. Nor is it obvious just what is meant by saying that experiences belong to a self.

The picture is muddled, but this does not prove that it is nothing but a muddle. It is still possible that this conception of language is enlightening and that, with some modification, it can be restated more clearly. Wittgenstein has shown that the picture must be rethought, but he has not shown that it must be entirely rejected.

(2) According to this picture of language the words refer ultimately to private experiences. Although the basic terms are verbally indefinable, each is associated with some particular sensation by repeating the word as one attends to the sensation. To understand a word is to know what it stands for, and misunderstanding is shown by using a word to refer to the wrong sensation. These words may be combined into sentences which describe experience. But how does one know when he uses any word that he is remembering this connection between word and sensation right ? He has no criterion of whether or not he is using the word to stand for the particular sensation which it is supposed to mean (258). This lack of any standard of the correct connection between word and sensation causes the picture to break down. If one has no criterion to tell him when he is using

a word to stand for the right sensation, then it makes no sense to say that a given word stands for this particular sensation, instead of any other (258). In what sense, then, does the word mean the sensation ? Moreover, if there is no criterion of whether or not a word is being used to stand for the right sensation, then one cannot speak of any distinction between correct and incorrect use (269). One cannot say that a person understands or mis-understands the language. Finally, if there is no criterion of whether or not a word correctly applies to a given sensation, then there is no way to judge the truth of any description of experience (260). But since every description claims to be true, genuine description is not possible in a language where there there is no standard of correctness.

But is it true that if a word refers to a sensation there can be no criterion of its correct use ? If memory has some credibility, one can check on his use of a word which stands for a sensation. He can arrange to have similar and contrasting experiences and see what he would call them. This is not like buying several copies of the same newspaper to verify the contents (265) ; it is more like buying copies of several different papers. One memory can support another provided that each has some initial probability. On the other hand, if memory has no credibility, one can have no criterion for his use of any word. Checking one's use against an external standard takes time and, therefore, requires the use of memory. The difficulty seems to arise from the rejection of memory rather than from the privacy of experience. At least Wittgenstein has not shown that it is the fact that a word refers to a sensation which makes any criterion of its correct use impossible.

Nor has he shown why such a criterion is necessary. Perhaps in some sense a criterion of correctness is presupposed by any distinction between right and wrong. But in what sense of the word " criterion " is this true ? Wittgenstein seems to assume that any genuine criterion must be something independent of that of which it is a criterion, external to the person using the criterion, and accessible to all persons equally. Until some reasons are produced for this view it remains a dogmatic assumption which may be questioned. In fact, there is good reason to reject this conception of objectivity, for it leads to an infinite regress. Suppose there were some external standard to which everyone could appeal. How would anyone know that he had checked his use of the word against the standard correctly (211) ? Are there standards for our use of the standards ? Unless at some point there is no longer any need of an *external* standard, all criteria become pointless.

15

(3) If words referred only to sensations as this epistemological picture requires, no one could teach his language to another (257). To explain one's meaning one would have to be able to show the other person the sensation which the word names, but the privacy of experience makes this impossible. Clearly, then, no public language could exist. The fact that we do communicate with each other shows that this picture of language is inadequate.

It seems to me, however, that there is a sense in which one person can show another what he means even when he means a private experience. Ostensive definition may require only qualitative similarity, not numerical identity, of sensations. A cannot show his own sensations of blue to B, but he can show him others of the same kind. A can present sensations of blue to B by directing B's attention to an object which looks blue to A. This assumes that, in the absence of evidence to the contrary, different selves have similar experiences when confronted with the same object.

Can this assumption be allowed ? Since no one can inspect the sensations of another, how could anyone be in a position to know that the sensations of other selves were similar to his own ? Suppose this cannot be known. On the verifiability theory of meaning the assumption would become meaningless. To me the assumption seems more plausible than the verifiability theory of meaning. And since Wittgenstein holds that, in terms of the picture of knowledge he is criticizing, the assumption that different selves have dissimilar experiences is possible although unverifiable (272), he must think that the assumption in question remains a possibility also. As long as it might be true that different selves have similar experiences when confronted with the same object, one person may be able to show another what he means even when he means a private experience. The privacy of experience makes the reality of communication questionable, but it does not entail that it is impossible. Wittgenstein has not shown, then, that the epistemological picture must exclude the possibility of communication.

But his point may be that what it fails to explain is not so much the possibility of communication as its knowability. Neither A nor B could ever be sure that the other was using the word " blue " to refer to the same kind of sensation that he was. What the privacy of experience would entail, if the assumption in question could not be established, is that we could never know whether or not we were in fact communicating with one another. Wittgenstein maintains, I think rightly, that in fact we often do know that communication exists. The picture of

knowledge cannot explain this unless it can give some reason to accept the assumption that different selves have similar experiences when confronted with the same object.

Clearly this assumption cannot be established by logical analysis, for it claims to make a factual assertion. Nor is it easy to see how it can be empirically established. Since one cannot inspect the experiences of another, he can have no inductive basis for the assertion that different selves have similar sensations. If the only way to support factual statements is by inductive generalization, then there can be no evidence for this assumption. I would argue, however, that inductive generalization must be supplemented by explanatory reasoning in any adequate picture of human knowledge. The way in which the discovered clues support the detective's hypothesis is not the same as the way in which known instances of a class support generalizations about the entire class. Factual statements may function to make experience intelligible as well as to predict it. To know our world we must systematize our detailed judgments under broad hypotheses and conceive the strange in terms of familiar models. The facts that are organized under and interpreted by an explanatory hypothesis are reasons which establish it. Since our experiences of apparent communication can be explained in part by the assumption that different selves have similar sensations, these experiences constitute evidence to support that assumption. Wittgenstein's claim that the picture of knowledge cannot explain how we know that communication exists rests on the presupposition that there are only two methods of establishing statements, logical analysis and inductive generalization. When this presupposition is rejected, his criticism fails to prove its point.

(4) The epistemological picture which we are considering holds that the ultimate evidence for our factual judgments is experience, but the given cannot be said to be ground for any statement. Does it follow from the sense-impressions which I get that there is a chair over there ? It does not follow from the sensations, for only a proposition can imply another proposition. It does not follow from the description of the sensations, for this does not logically entail the perceptual judgment. Nor is the relation like that non-logical inference from the marks on the rug to the fact that the chair was there ; no transition is involved (486).

Why does Wittgenstein assert that no perceptual judgment could follow from sense-impressions ? If his point is simply that in ordinary language the word " implies " is used for a relation between propositions only, he may be right. But surely

one may depart from ordinary usage when a more technical
vocabulary seems required. If his point is that it is misleading
to use the same word for the very different relations of proposition
to proposition and of sensation to proposition, I am rather dubi-
ous. Certainly it would be misleading to suggest that the two
relations are the same in every respect, but there may be suffici-
ent analogy between them to make the comparison enlightening.
The relations of experience to description and of premise to
conclusion do share the features necessary for the first term to
justify the second term in each case, and this is the crucial
point here. What is needed is an analysis of justification to show
what is required for something to justify a judgment. Neither
Wittgenstein nor his opponents have yet done this adequately.

I can agree that the description of my sense-impressions of the
chair does not logically entail that the chair is there. I can also
agree that the relationship is not like the non-deductive inference
from one fact to an entirely different fact. This does not imply,
however, that there is no sense in which the description supports
the judgment. The description of my sense-impressions does
provide some justification for my judgment that the chair is there,
although the nature of this justification needs considerable
clarification.

(5) This picture of knowledge holds that the most basic judg-
ments are those which describe experience, but it is misleading to
say that one " describes " his sensations (290). If the meaning
of a word is its use, to understand description one must under-
stand how descriptions are used in our language (291). The
sentences which normally function as descriptions in ordinary
language, statements about physical objects, are used for a
number of purposes, none of which could be served by " descrip-
tions " of sensations. They are used for recording what one has
learned, informing others, stating truths or falsehoods, and
expressing conviction. Yet one cannot be said to learn about
his sensations; he simply has them (246). No " description "
of sensations could be informative, for it would be in a private
language (243). Nor could it be said to be true or false, since
there could be no criterion of correctness in such a language (258).
And since doubt about the given is out of place, it makes no sense
to claim certainty here either (246). Only muddled thinking
could confuse the language of sensations with ordinary description.

On one point Wittgenstein is entirely correct : we need a
careful analysis and comparison of descriptions of physical
objects and descriptions of sensations. I do not believe, however,
that such an investigation will show them to be as different as he

suggests. The differences which he mentions seem either unimportant or non-existent. I do not see why one cannot record his sensations in spite of the fact that he has not learned about them over a period of time. I have already argued that the privacy of experience does not necessarily imply the impossibility of communication and that the claim to objective validity is ultimately independent of external criteria. Finally, I will not admit that doubt *about* the given is out of place. The given itself cannot be doubted, for it makes no claim that could be questioned. Yet a description of the given can be doubted. Consider the test of vision in which the optometrist asks the subject which of two crossed lines looks darker. As the subject looks through various lenses he often has to hesitate before answering. His doubt is not over which line actually is darker, but over which appears darker. Nor is the subject unfamiliar with the meaning of words like " line ", " darker ", or " vertical ". The subject simply cannot be sure about the character of his experience. Wittgenstein has not shown that it is all a mistake to talk about descriptions of experience.

(6) If we picture experiences as objects to be described, they drop out of our language game as useless. One does not need to feel another's pain in order to know that he is suffering (246). The correctness of one's judgment that his own tooth is decayed depends upon the results of X-raying or drilling and not on the ache he feels (270). Epistemologically it does not matter what sensations another has, or whether he has any, since one could never inspect them (293). It does not even matter what sensations one has oneself, for he can never be sure that he is remembering rightly which is which (271). Far from being the basis of all knowledge, these private objects are irrelevant to our knowledge claims.

Each of the examples Wittgenstein uses to show the irrelevance of private sensations seems questionable. No doubt one can often have good reason to say that another is in pain, but can his grounds be as conclusive as those of the person who feels the pain? The judgment that one's tooth is decayed will be tested by X-ray and drill, but the ache one feels is his present justification for making the judgment. One can never have the sensations of another to justify his judgment, but they are still relevant to its correctness. (Note how Wittgenstein assumes that truth, verifiability, and justifiability are not simply related, but inseparable.) Public objects as well as private sensations become uncertain bases for judgment if memory cannot be trusted.

These examples hardly establish the complete irrelevance of

private sensations to knowledge, although they do suggest that
they are sometimes unnecessary. Actually, if the epistemolo-
gical picture is correct, sensations are always essential. There
are no public objects to be data for knowledge ; the only objects of
which we are conscious are private experiences. Wittgenstein's
reasoning is circular here, for he begins by presupposing a know-
ledge of public objects which would be impossible if the picture
he is criticizing were correct.

(7) This picture of knowledge cannot even be stated in meaning-
ful terms. It wishes to assert the existence of private experience,
but this cannot be significantly said. Since it is admitted that
no one can really know what kind of sensations another has,
no one can say that others have sensations at all. It is pointless
to assert the existence of something unless one can describe it to
some extent, for *what* is it that is said to exist ? (294). More
surprising, no one has a right to say that his own sensations exist.
Since the word " sensation " is a word in our public language, its
use requires a justification which everyone can understand.
However, no one can exhibit his sensations to another (261).
Thus one can assert neither the existence of others' sensations
nor the existence of his own.

I agree that if one knew absolutely nothing about the nature
of the sensations of others, it would indeed be pointless to assert
their existence. But in calling them " sensations " we already
describe them as possessing the generic characteristics of mental
phenomena. Wittgenstein has also assumed too readily that
nothing specific can be known of the sensations of another ;
this epistemological picture need not lead inevitably to such
scepticism. Even if it did, this would not imply that one cannot
significantly say that others have sensations, unless one assumed
the verifiability theory of meaning.

Nor is it really proved that one cannot rightfully assert the
existence of his own sensations. The fact that the word " sensa-
tion " is part of our public language shows only that its meaning
must be shared, but not that its justification must be possessed
by all. (Note how often Wittgenstein dogmatically presupposes
the verifiability theory of meaning even though one of his main
theses is its inadequacy.) I have already suggested that the
privacy of experience need not imply the impossibility of a
public language if different minds have similar experiences when
confronted with the same objects.

(8) But, one would like to object, these arguments count as
nothing in the face of one's immediate consciousness of the given.
How can Wittgenstein deny the existence of experience when there

it is confronting one's consciousness ? He is not denying the obvious fact that we all have experiences, but only rejecting the way we talk about them (304). We are misled by language into thinking that sensations are a peculiar kind of object which we observe and describe ; we interpret a grammatical difference as a difference in subject matter (401). This picture of experience, language, and knowledge is simply a meaningless muddle.

In a way these considerations add nothing new to Wittgenstein's argument. His contention that this picture is a muddle arising from linguistic misunderstanding is worth only as much as the support he has given it in other places ; no new evidence is produced here. Yet a new twist is introduced in these considerations, for he is trying to clarify the sense in which he takes issue with those who hold the picture.

To them he appears to be assuming the reality of physical objects and denying the existence of states of mind. It seems preposterous to deny the one kind of object of which we are directly aware ; this contradicts the most compelling evidence of our senses. These people think of the issue as, in a broad sense, factual. Wittgenstein is contending that the issue is, in an equally broad sense, logical. He is not rejecting their picture as false, but as meaningless. Thus any appeal to the fact that we do see colours and feel pains, far from being a conclusive answer to his criticisms, is quite irrelevant.

Wittgenstein's contention that the issue is purely linguistic rests on his conception of the nature of philosophy. This is not the place to explain in detail why I cannot accept this view. Certainly philosophical questions are not like questions about tomorrow's weather ; but neither are they like questions about what is implied by a set of premises. The usual distinction between factual and non-factual issues is inapplicable to philosophical problems ; these are questions about how to talk about the facts most adequately. To such questions the facts are not irrelevant.

But while the facts of experience are relevant to the issue, Wittgenstein is certainly right that the mere fact that we do have experiences does not settle the matter. Those who hold the picture in question must show that it is an adequate conception of the role of experience in knowledge, and a confused picture can hardly claim adequacy. Hence each of Wittgenstein's detailed criticisms must be met either by refuting his reasoning or by reformulating the picture. It is the adequacy of the picture, and not the existence of the experience, which is at issue.

(9) The part of the epistemological picture which Wittgenstein is most anxious to repudiate is the privacy of experience. He contends that statements like " Only I can know whether I am in pain " are either false or nonsense. If " know " is used in the ordinary sense, others can sometimes know whether I am in pain (246). When I am writhing on the floor, screaming loudly, with a broken bone protruding from my arm, one is quite justified in saying " I know that he is in pain ". One could, of course, *say* only " I believe that he is in pain ", but this would be a verbal change which expresses no real doubt (303). On the other hand, if " know " is used in the philosophical sense implying certainty, statements like " Only I can know whether I am in pain " are nonsense. I cannot be said to be certain of my sensations, for I cannot doubt them (246). It is senseless to lament the fact that another person cannot be aware as I am that I am in pain, for it would be impossible for one person to feel the pain of another (253). Consequently the complaint loses its sceptical point by becoming an empty tautology.

Wittgenstein is doubtless right in claiming that, in the ordinary sense of " know ", one person can sometimes be said to know that another is in pain. However, ordinary language may be misleading here by obscuring the weakness of the grounds for such judgments. Surely a more restricted use of the word " know " is not ruled out merely because it departs from ordinary usage.

Wittgenstein's reply is that while the word may be used in any sense the sceptic desires, the philosophical use of the word reduces the sceptic's statement to an empty tautology (248). I must admit that I have never been quite sure of the point of this reply ; it is a curious argument which would refute scepticism by showing that it is necessarily true. Perhaps the argument hinges on the theses that all necessary truths are analytic and that all analytic statements are nonsense. The former is probably false, unless made analytic, and is certainly in need of proof. The latter is relatively innocuous, since it claims, unless made preposterous, that analytic statements are meaningless only in the sense that they are not empirical predictions. In any event, one would have expected Wittgenstein to have discarded these positivistic dogmas of his youth.

Or perhaps the argument hinges on the more plausible thesis that to assert anything is to deny its alternatives (251). To say that the book is red is to say that it is not blue and not green, etc. If one did not rule out any colours by his statement, he would not be specifying anything at all about the colour of the book. Thus the significance of any assertion depends upon the alternatives

which it excludes. The statement that no one else can know my pain is significant only if it excludes the possibility of my pain being known by another person. But what would it be like for another person to know my pain ? We cannot specify any alternative to ignorance here. What is it, then, that the sceptic is denying ? If he is denying nothing, his statement that everyone else is ignorant of my pain becomes vacuous.

The nature of necessary truth is obscure at best. But at least it is clear that if this argument holds against scepticism, it is equally devastating against all necessary statements. Wittgenstein was too much interested in logic and mathematics to deny all significance to necessary truths ; he must mean only that they are not significant in the same way as empirical truths. However, the sceptic need not be disturbed to learn merely that his position is not an empirical generalization. No sensible sceptic pretends to be a scientist.

My review of the considerations which led Wittgenstein to reject this picture of factual knowledge is now complete. I must admit, however, that I have not always found his reasoning easy to understand. This is partly because of his style of writing, but even more because of the originality of his thinking. It is quite possible, therefore, that I have not done him justice. If this be the case, I call upon his many followers to explain to me, and to others as perplexed as I, exactly why this picture must be discarded. Until this is done I can only conclude that Wittgenstein has not proved his case. He has, and this is an important achievement, shown that there is much confusion in this picture. He has not, however, shown that it is nothing but confusion. The egocentric predicament is still a real one for any philosopher, although the picture upon which it is based needs considerable revision.

Many fundamental questions remain : What is experience ? Are the objects we experience public or private ? In what sense, if at all, is experience a ground for judgment ? What is justification ? In what way, if at all, does language derive its significance from experience ? What is meaning ? Neither the partisans of this epistemological picture nor the followers of Wittgenstein should take it for granted that these questions are already settled in their favour. Real progress requires that open questions be approached with open minds.

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VIII.—THE MODAL "PROBABLY"

By C. L. Hamblin

METRICAL probability-theory is well-established, scientifically important and, in essentials, beyond logical reproof. But when, for example, we say " It's probably going to rain ", or " I shall probably be in the library this afternoon ", are we, even vaguely, using the metrical probability concept ? Is what we say about statements like these even *consistent* with their interpretation as metrical probability-statements, such as that the degree of probability of rain or of my being in the library is greater than some figure x, with all that this involves in the way of rules for arithmetic operations on the numbers representing degrees of probability ? I want to suggest that, contrary to what is usually supposed, there is a case for answering these questions in the negative.

The argument involves formal considerations which I shall first sketch briefly. I derive a " modal " logic of probability from metrical probability-theory, based on the following definition : " p is probable " means " the probability of p is greater than or equal to x ". The theorems of this logic will be taken to be those statements which are true for any value of x between $\frac{1}{2}$ and 1 for any consistent allocation of probabilities to the statement-variables.

I turn next to a concept analogous to one proposed by Shackle,[1] and applied by him to certain problems in economics : it may be termed " plausibility ". In terms of a similar allocation of numerical indices to statements a modal logic of " plausibility " is developed. The numerical indices are here, however, *non-metrical* in character : that is, no arithmetic properties are involved. It can in fact be demonstrated formally that " p is plausible " can be interpreted simply in the form " p does not occasion surprise "—a kind of reduction which is not possible in the case of the already-introduced logic of " probable ".

A possible rival contender as a definition of " p is probable " is, however, the following : " p is *more plausible than* any contrary assertion ". Thus a rival modal logic of probability can be set up. It is found to differ at certain key points from the logic based on a metrical theory. I hope finally to show that, on the points in dispute, the second system is at least the equal of the first as an account of the ordinary use of the word " probably ".

1. As basic logical equipment we shall need a restricted form of the usual modal logic of necessity and possibility, namely, that part which does not involve iteration or " nesting " of modal operators. We need, that is, to be able to refer to the necessity or possibility of statements of an ordinary two-valued system but not to the necessity or possibility of statements which are themselves modal. Because of its connection with probability we shall also need " epistemic " logic, in the sense of von Wright.[1] A distinction can, however, be made between what are usually called " apriori " and " aposteriori " probabilities : the former are the probabilities in the absence of prior knowledge of a situation, the latter the probabilities when such knowledge is taken into account. In accord with von Wright's terminology these are " alethic " and " epistemic " probabilities respectively, and recall his distinction between alethic and epistemic senses of the word " possible ". The present account deals mainly with " epistemic " probability, which is the more general of the two.

The properties of the resultant logical system can be represented on a picturable *model* of a logical language, which I shall call the *class-model* (see figure). In a finite statement-language built on

	True			Known false	
S.d's.	s_1	s_2..........s_k		s_{k+1}..........s_n	
Probabilities	a_1	a_2	a_k	0	0

elementary statements $p_1, p_2, \ldots p_m$ any statement, as is well known, can be represented in a form called " complete disjunctive normal form ", *i.e.* as a disjunction of *state-descriptions*. A state-description (s.d.) is a conjunction which contains, for every elementary p_j, either p_j itself or its negation : any such conjunction is in effect a complete specification of the logical universe in question. If alternatively we *begin* by assuming that we are given an exhaustive set of s.d's, say $s_1, s_2, \ldots s_n$, we can regard any statement as a *class* made up of these, namely, as the class of

[1] G. H. von Wright, *An Essay in Modal Logic* (Amsterdam, 1951). The present system interrelates von Wright's $M_1 + M_e$ and $V_1 + V_e$.

those s.d's which appear in its normal expansion. Logical properties of statements may then be represented as class-properties : the negation of a statement is the complementary class, the conjunction of two statements the " intersection ", and so on. In particular, necessary statements are equivalent to the full class.

Of the s.d's, one and only one, say s_1, can be regarded as " true ". Thus a *true* statement will be any that contains s_1. The logic of " truth " and " necessity " can be derived on the basis of these identifications from the logic of classes. For example, it is clear that any necessary statement must be true.

" Knowledge " about the universe in question consists in having eliminated certain of the s.d's, say $s_{k+1}, \ldots s_n$, as " known false ". (Since what is " known to be true " must be " true ", these must not include s_1.) A statement is " known to be true " if it contains all the remaining s.d's, namely, all of s_1, $s_2, \ldots s_k$. Hence epistemic logic can be set up on the same basis.

Finally, we can regard the s.d's as bearing indices of probability $a_1, a_2, \ldots a_n$ respectively. We need not consider how these probabilities are obtained : theorems of probability-logic will be those which hold for any allocation of real numbers, subject only to the restrictions that they are non-negative and add to unity ; and in the case of an " epistemic " system, that the probabilities $a_{k+1}, \ldots a_n$ associated with " known false " s.d's are zero. In particular, no special restriction attaches to a_1, the probability of the " true " s.d. Since s.d's are mutually exclusive their probabilities are additive, and the probability of any statement is given by the sum of the probabilities of the s.d's it contains. Thus we also have a model of the logic of probability. The only remaining step is to define " p is probable " as " the probability of p is greater than or equal to x ". Theorems concerning this " modal " probability are those statements which obtain independently of the value of x, provided it lies between $\frac{1}{2}$ and 1.

We are thus in effect concerned with the relations between statements of the forms : " $T(\alpha)$ " (α is true) ; " $L(\alpha)$ " (α is necessary) ; " $V(\alpha)$ " (α is known to be true) ; and " $P(\alpha)$ " (α is probable)—where α is in each case a non-modal statement. The usual two-valued operators can be used to operate on these, as well as to indicate the form of α. It is clear, however, that if Polish notation is used the brackets may be omitted ; and also that any ambiguity which arises from the omission of " T " is logically unimportant. Hence the usual notation can be used.

The following is a characteristic axiom system :[1]

[1] Proofs of this and similar assertions will be published elsewhere.

(i) A set of axioms for the two-valued system, with the usual rules of substitution, adjunction and detachment, except that substitution must not involve nesting of modalities ; (ii) for the logic of necessity, the additional rule " If α is a non-modal theorem, $L\alpha$ is a theorem ", and the additional axioms (1) $CLpp$, what is necessary is true, and (2) $CLCpqCLpLq$, if it is necessary that p materially implies q then if p is necessary q is necessary ; (iii) for epistemic logic, (3) $CLpVp$, what is necessary is known, (4) $CVpp$, what is known to be true is true (—in view of (3) and (4), (1) may be omitted—), and (5) $CVCpqCVpVq$, if it is known that p materially implies q then if p is known q is known ; (iv) for probability (in the " epistemic " sense), (6) $CVpPp$, what is known is probable, (7) $CPNpNPp$, if Np is probable p is not probable, and (8) $CVCpqCPpPq$, if it is known that p materially implies q then if p is probable q is probable. For an " alethic " probability system, axioms (6) and (8) must be weakened to $CLpPp$ and $CLCpqCPpPq$ respectively.

2. Some of the features of this system call for comment in their own right and independently of the main trend of the present argument. One of these is the lack of any direct relation between probability and truth. It has sometimes been supposed, presumably from misinterpretation of arguments such as that if a statement is *taken* to be true it must be *taken* to be probable, that it must be the case that whatever is true is probable $(CpPp)$. This is self-stultifying : in contrapositive form it says that anything that is not probable is false $(CNPpNp)$. It is still true, however, that what is *necessary* must be probable $(CLpPp)$; and, in an epistemic system, that what is *known* is probable $(CVpPp)$. The latter, in particular, seems to represent what is being aimed at.

Secondly, we might notice the distinction between *improbability* (PNp) and *non*-probability (NPp). This is a distinction which is as a rule only imperfectly made in ordinary speech : we incline to regard " probable " and " improbable " as contradictories. At the same time we find it quite intelligible that a statement should be neither probable nor improbable, *i.e.* neutral. Moreover we must remember here that the present " probable " is to some extent doing duty for a range of concepts : even in ordinary speech probability admits of degrees, and we certainly do not regard, say, " highly probable " and " highly improbable " as contradictories.

So far as conjunction and disjunction are concerned there are inequalities in ordinary metrical probability-theory which lead

directly to theorems here. Thus we have $CPpPApq$, $CPKpqPp$
and various others. But I want to turn attention particularly to
the expressions $CKPpPqPKpq$ (" if p and q are both probable
their conjunction is probable ") and $CPCpqCPpPq$ (" if it is
probable that p materially implies q then if p is probable q is
probable "). Neither of these is a theorem, and in terms of the
metrical theory we can see why : two events which both have
high probabilities on (say) a frequency basis may be " anti-
correlated ", so that their joint probability is somewhat lower.
Yet we not infrequently employ just such reasoning in ordinary
applications : " The murder-weapon was probably a stiletto and
it probably pierced the heart ; hence death was probably im-
mediate." Does anyone object to this kind of inference ?

3. Without further discussion at this stage I shall proceed to
outline the alternative system.

Shackle [1] has discussed in detail the reasons for preferring, in
most real-life situations involving a choice between rival hypothe-
ses, a measure of worth of the hypotheses concerned which
differs in several respects from the probability-measure usually
adopted by theoreticians. I shall not here repeat these reasons :
it is enough to say that we want a measure which (i) can be
allocated to a given hypothesis irrespective of the number and
measures of contrary hypotheses, and (ii) is not limited by the
usual probability addition-rule. I shall call this measure
" plausibility ". Strictly speaking, it is not a measure at all,
since we are interested not in arithmetical properties but only in
topological ones ; but it is convenient to regard it as ranging like
probability from 0 to 1. Indices $b_1, b_2, \ldots b_n$ of " plausibility "
can be allocated to the s.d's in our class-model subject only to the
following further restrictions : (i) that at least one s.d. must have
the index unity, and (ii) that in an " epistemic " system (—since
we have here as before a choice between " alethic " and " epis-
temic " senses—) the plausibilities $b_{k+1}, \ldots b_n$ of the " known
false " s.d's are identically zero.

In place of the addition rule for probabilities we have the
following : the plausibility of any disjunction is equal to the
largest of the plausibilities of the components. Hence the
plausibility of any statement can be obtained in terms of the
indices allocated to the s.d's. Finally, we define the modal " p is
plausible " as " the plausibility of p is greater than or equal to y ",
where y may take any value from 0 to 1.

[1] *Loc. cit.*

Writing " $Q\alpha$ " for " α is plausible " we have the following axiom set : in addition to the basic axioms (2)-(5) above, (9) $CVpNQNp$, if p is known Np is not plausible, (10) $CNQNpQp$, if Np is not plausible p is plausible, (11) $CVCpqCQpQq$, if it is known that p materially implies q then if p is plausible q is plausible, and (12) $CQApqAQpQq$, if the disjunction of p and q is plausible then either p is plausible or q is plausible. The last two axioms may be replaced by a single somewhat less elegant one, $CNQNCpqCNQNpNQNq$.

Once again we have no direct relation with " truth " : that is, it is not a theorem that " what is true is plausible " $(CpQp)$. From (9) and (10) we have, however, $CVpQp$, " what is known is plausible ". Axiom (10) specifies that a statement and its negation cannot both be implausible ; but its converse $CQpNQNp$ is not a theorem—any or all of a set of alternatives may be plausible. It follows that a conjunction of plausible alternatives is not necessarily itself plausible : it could be inconsistent. The same kind of reasoning rules out the strengthened form $CQCpq$-$CQpQq$ of (11).

An identical system results from the definition of " Qp " as " $NVCpS$ ", where S is a constant statement subject to the axiom NVS.[1] We can interpret S as asserting " surprise " : " I should not be surprised if p were the case." Formally, S can be represented in the class-model as the class of s.d's of plausibility less than y.

We now come to the alternative definition of " probable " : we identify " p is probable " with " p is more plausible than anything else ", or equivalently " p is more plausible than Np ". This is not definable in terms of Q ; but it is definable on the same model (with indices $b_1, b_2, \ldots b_n$) and hence can be related to Q. As axioms we omit (6), include (7) as before, strengthen (8) to (13) $CPCpqCPpPq$, if it is probable that p materially implies q then if p is probable q is probable, and add (14) $CNQNpPp$, if Np is not plausible p is probable. From (7) and (14) we can prove $CPpQp$, whence (10) may also be omitted as unnecessary. These axioms are also characteristic for the definition of " Pp " as " $LCNpS'$ " (" I should be *very much* surprised if p were false ") where S' is a constant statement related to the previous S by the axiom $CS'S$.

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[1] This method of defining a modal operator has been applied to " deontic " logic by A. R. Anderson, " The formal analysis of normative systems ", Tech. report no. 2, Contract SAR/Nonr-609(16), U.S. Office of Naval Research, New Haven (1956). It can also be used in the present system to define " V ".

4. Axiom (13) is one of the controversial expressions mentioned above; and the other, $CKPpPqPK\bar{p}q$, can be derived as a theorem. The fact that these hold in this system but not in the system based on metrical probability may be taken as typifying the difference between the two systems. Which system is the more reasonable?

The first thing I want to suggest is that before the advent of metrical probability-theory in the seventeenth century few people if any would have dissented from the reasoning these expressions embody, and that it is only since then that we have become, as it were, actuarially-minded in our contemplation of the unknown. In many ways, of course, this has been a good thing: rather than simply accept a translation of "probably p" as "there are reasons for expecting p" we are encouraged to go on and ask "Just how strong are those reasons?" And in many cases—those, for example, in which the frequency theory can be applied—we find we can get an answer.

The real trouble starts when reasons conflict—when there are both reasons for expecting p and reasons for expecting Np. These may be of a kind that it is impossible to reduce to anything resembling the frequency theory: the best we can do is "weigh" reasons against one another. The situation is, as precisely as I can make it in a simple theory, that envisaged by the allocation of indices of "plausibility". Since these are non-metrical in character, it is irrelevant to object that one cannot "weigh" reasons—this would simply be to say that one cannot come to a conclusion by any means at all short of tossing a coin. And even this latter contingency is allowed for in the logic to some extent, by the case in which both alternatives are fully and equally plausible.

It may be objected that this is a theory of "subjective" probability. It does, it is true, accommodate the case in which the weighing of reasons is a subjective process, or in which the "surprise" which would follow the occurrence of the unexpected alternative is literal psychological surprise rather than "rational" (?) surprise. But there are objective ways of weighing reasons, and they are not necessarily actuarial in nature. The assertion that there are not can, I think, be reduced to the truism that it is always possible to reject a rational argument.

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ON THE SOUL : A DISCUSSION

THE aim of a recent book of essays [1] by a group of Oxford dons is to examine from a Christian standpoint the bearing of contemporary philosophy upon Christian Doctrine. The chapter contributed by Mr. J. R. Lucas entitled "The Soul" interested us particularly and is the subject of our paper. In it the author is concerned mainly to attack the arguments of those philosophers (Ryle is the arch-enemy) who contend that the notion of the soul as traditionally understood is logically improper and that in its most commonly used legitimate sense the word can be analysed out in terms of patterns of behaviour. His chief task, he says, is to expose the mistaken assumptions upon which such arguments proceed and "to show only that the term is permissible, not that it is obligatory" (p. 132). At the end of the essay he attempts to give "a more positive account of what we mean in our different uses of the word ' soul ' " (ibid.).

Now the analysis of such an important but elusive concept by the use of modern methods is a most commendable undertaking, and one which has been neglected too long. That we feel dubious about parts of his essay does not prevent us from believing that Lucas has been successful to some extent in his task : for, not only has he pointed out some of the senses in which the term ' soul ' is permissible, but, more vitally, he has established the inadequacy on at least one count (and perhaps two) of the doctrine of logical behaviourism as ordinarily understood. Yet in the course of the essay it becomes obvious that Lucas has little sympathy with, and indeed misunderstands, much that has been accomplished recently in philosophy. This lack of sympathy is shown, not only in e.g. his treatment of Ryle's *The Concept of Mind*, but quite clearly in his so-called "positive account" of the soul ; here he seems to abandon altogether the analytical approach and to make statements which are more like affirmations of faith than contributions to logical analysis. In short, he does not succeed in the more difficult task of elucidating the religious sense of the word ' soul '.

I. *General arguments against logical behaviourism* (pp. 135–140)

Lucas makes some useful and valid points in the early stages of his essay (pp. 135–138). For example, many contemporary philosophers would agree with his emphasis on the role of behaviour in the imputing of ' spiritual ' qualities, although they might wonder whether this approach is consistent with what he says later when he speaks of the soul as " beyond personality " and " beyond morality ".

[1] *Faith and Logic*, edited by Basil Mitchell (Allen and Unwin Ltd.), pp. 222, price 21s.

But especially interesting is his analysis of why logical behaviourism is a " shocking thesis ". This is said to be (a) because it is supposed that, if the criteria of applicability of a term are given, and if these criteria are behaviouristic, then ' spiritual ' and ' mentalistic ' terms used according to these criteria have been ' reduced ' to behaviour ; in other words, criteria of applicability are confused with meaning; and (b) because there is a confusion between ' behaviour ' when it does not include speech (what we might call the ordinary sense of ' behaviour ') and ' behaviour ' when it does include speech (what one might call a stretched or philosophers' sense of ' behaviour '). Lucas, indeed, later develops what is essentially point (a) as a basic reason for rejecting logical behaviourism ; and this we shall have to consider in due course.

But first Lucas attacks Ryle for what he calls " an unfortunate doctrine about the nature of language, which seeks to divide concepts into a set of mutually exclusive category baskets, divided from one another by a great gulf so that words that wanted to move from the one category to the other could not do so " (p. 138). It is doubtful, however, whether Ryle, or anyone, ever held such a view, especially in the form of " . . . if a word is sometimes categorially different from another word it is always so " etc. (p. 139). Ryle would probably agree that " whether a conjunction or disjunction of two terms is categorially permissible or not is a question which must be decided in each case individually " (p. 139). Indeed, *The Concept of Mind* might well be looked upon as Ryle's elucidation of the respects in which ' body ' and ' mind ' and related terms are and are not categorially similar—even though it does turn out that they are categorially *more* dissimilar than ' Oxford Colleges ' and ' Oxford University '. But doubtless Ryle would permit certain conjunctions and disjunctions of various ' mentalistic ' terms on the one hand and ' material ' terms on the other hand (*e.g.* ' he is six feet tall and imaginative '), provided it were recognised that the two terms were imputed to the individual on the basis of rather different kinds of criteria.

The author's second attack on Ryle is for " the equation of meaning with method of verification " (p. 140). Now first, it may be queried whether Ryle ever put forward such a view in *The Concept of Mind*. But even if it is agreed that he did, it needs to be made clear that what is being discussed is *factual* meaning : to make an elementary point, no one would suppose that the meaning of moral or mathe-matical statements is given by the method of their verification, or even that they can be verified at all in any ordinary sense. Never-theless, we are here, in a certain sense, in agreement with Lucas—and with Hampshire, who originally made this point when reviewing *The Concept of Mind* (see MIND, lix (1950), 246). For we agree that equation of factual meaning with method of verification does not enable us to give a plausible account of statements about subjective experiences : it does indeed drive us into logical behaviourism,

at least if ' verification ' is construed in its usual way as resulting
from exteroceptive, sensory observation. But we would emphasise
that, in our view, factual (and, *a fortiori*, scientific) statements need
to be, at least in principle, intersubjectively confirmable ; for a
statement to qualify as factual we need to know what would count
as its confirmation or disconfirmation. And to say this is not to
equate factual meaning with method of verification. Further, we
would argue that within the class of factual statements there is
contained the sub-class of statements about subjective experiences ;
from which it follows that such statements are, in some sense,
intersubjectively confirmable. Of course, to say that subjective
statements are intersubjectively confirmable may be construed as
self-contradictory. But all that is meant is that there are always,
in principle, logically possible conditions under which we should
unhesitatingly reject reports of subjective experiences ; and other
logically possible conditions under which we should unhesitatingly
accept them. In practice, both these types of conditions may obtain
very rarely, if at all, so that we have to put up with (empirical, not
logical) uncertainties and probabilities. And this is a point which
assumes considerable methodological importance to psychologists
who try to study subjective experiences.[1] Perhaps, in all this we are
saying nothing more than Lucas himself would be prepared to admit.
No more than he do we wish to confuse the criteria of applicability
of a term with its meaning ; and not least is this so in the case of
words which refer to subjective experiences. But, as we shall try to
show, we very much doubt whether the consequences which Lucas
extracts from his rejection of the verificationist theory of meaning
and of logical behaviourism are those which validly follow from it.

II. *Specific arguments against logical behaviourism* (pp. 140–145)

Next, the four reasons Lucas gives for rejecting the equation of
meaning with method of verification must be considered. These will
be taken in turn and analysed in detail. An evident preliminary
point, however, is that all the four reasons advanced are directed
against logical behaviourism rather than against the theory of
meaning from which the doctrine is said to flow. It is not quite made
clear on page 140 whether one or both or one simply as a consequence
of the other, is being attacked ; but it is evident from what follows
that the enemy is a specific rather than a general theory of meaning.

(i) *An argument from phenomenalism* (pp. 140–141)

" Logical behaviourism is on a level with phenomenalism ; and the
reasons for rejecting the latter may be turned also against the former "
(p. 140). This has a certain plausibility. One important difference
between the two cases, however, is this : the kinds of event or object
(or whatever : in general, organisms behaving) to which psychological

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[1] For an interesting account of recent psychological work in this field
see P. McKellar, *Imagination and Thinking* (London, Cohen & West, 1957).

words are reduced are different from those to which material object
words are reduced, *i.e.* sense-data ; for there is what one might term
an easy and natural intersubjective confirmability about statements
containing the behavioural terms to which psychological words are
reduced by logical behaviourism : there is, in general, agreement
between different observers in the same spatio-temporal position
about what ' raw behaviour ' is actually going on ; were this not so,
there would, indeed, be no point in recommending the reduction.
For it is just because there is a high degree of intersubjective agree-
ment about statements concerning ' raw behaviour ' that complexes
of the states of affairs to which these statements refer can be said to
serve as criteria for the imputation of more complicated psychological
terms—about statements containing which there is rather less
" easy and natural intersubjective confirmability ". But in the case
of material object words, the terms to which (*i.e.* sense data) they are
reduced have no known conventional usage at all : we could, if we
wished (though this would be very inconvenient), talk of humans
solely in terms descriptive of ' raw behaviour ' episodes. In other
words, complexes of behaviour episodes occurring in certain situations
may, in a quite genuine way, serve as criteria for the imputation of
psychological states and processes ; complexes of sense-data, on the
other hand, since their occurrence and non-occurrence are not pub-
licly confirmable, cannot in the same way serve as criteria for the
assertion of material object statements.

The first argument against logical behaviourism may also be
considered from another point of view. Lucas writes, " . . . we may
urge that the extreme diversity of sorts of behaviour we are prepared
to accept as criteria for the application of a single mental or spiritual
concept would be unreasonable except upon the hypothesis of some
unifying principle " (p. 141).   Does this mean that the theory that to
talk about the nature of mental or spiritual qualities comes down in
the end to talk about how humans behave, is unreasonable ?   If it
does, then what does ' unreasonable ' mean here ?   Perhaps the
point that Lucas is making, and with which many people would agree,
is that logical behaviourism 'feels' wrong, and this is because in ' redu-
cing' statements about the mind or soul to statements about behaviour
something *seems* to be left out : so in comes what is called a ' unifying
principle '.   Lucas puts it thus : " . . . we feel that it is the same
quality which is being discovered under these various guises ; there
is an intuitive feeling of consistency about our varying judgements "
(p. 141).   All this might be true : perhaps everyone (including the
logical behaviourist) feels compelled to posit an underlying substance
or a ' unifying principle '.   But need this fact (if it is one) worry the
logical behaviourist ?   According to him, nothing *is* left out on his
theory, for any statement about a quality of mind or soul can be
translated into its logical equivalent, *i.e.* into statements about
behaviour. The compulsion, if there is one, that is felt to posit a
' unifying principle ' has, no doubt, interest for the psychologist : it

does not affect logical behaviourism as a philosophical theory : for the thesis of the logical behaviourist is, in a broad sense, logical and must be examined from a logical point of view. Lucas, in fact, at this point gives us not logic but psychology.

(ii) *The attack from ' open texture '* (pp. 141–142)

Lucas now uses against logical behaviourism an argument similar to the one brought against phenomenalism by Dr. Waismann,[1] *i.e.* an argument from the ' open texture ' of empirical concepts. " There is ", writes Lucas, " no set of statements about behaviour which *entail* or *are entailed by* any statement about mental or spiritual characteristics. However carefully, fully and exactly we specify our tests beforehand we shall never be able decisively to discriminate between τὸ εἶναι δίκαιος and τὸ δοκεῖν εἶναι δίκαιος, being honest and seeming honest." He continues, " There is as it were a Naturalist Fallacy, or rather a Cephalus Fallacy, in any proposed definition of mental or spiritual concepts in behaviouristic terms ; the definition, though often true, is true only *ceteris paribus ;* and it is always possible by considering unusual circumstances to find exceptions to it."

It is not our intention to discuss here the merits or demerits of this reasoning. But even if for the sake of the argument we grant Lucas this point, namely that the ' open texture ' of mental or spiritual concepts rules out the possibility of any translation of them into the language of behaviour, it may still be asked what follows from this rebuttal of logical behaviourism ? How does this conclusion help to establish the legitimacy of ' the soul ', especially in the sense indicated by Lucas in his so-called " positive account " ? Lucas himself believes that this argument is one of the " grounds for maintaining that men after all do have souls " (p. 145). Now it is clear from the context that he is here using ' soul ' metaphysically. He writes : " Thus, in the fused phrase ' —have souls ' we may or may not be using the word ' soul ' to *include* mind. In the preceding arguments (against logical behaviourism) we have been using the word in the *inclusive* sense . . . " (p. 146 : our italics). The implication of such language is that soul is more than mind, and this probably suggests to many readers that, apart from mind, there is another element that is outside the range of human experience : that is, even when he says he is discussing the problem of other minds, Lucas insinuates (perhaps unwittingly) a metaphysical sense into ' soul '. This we feel he cannot legitimately do by simply refuting logical behaviourism. It does not follow, for example, that because statements about mental and spiritual qualities somehow transcend their evidence (as the argument from ' open texture ' purports to show), in the sense that they are not merely redescriptions of it, we are

[1] F. Waismann, ' Verifiability ', *Proceedings of the Aristotelian Society,* Supp. vol. xix. 119–150, reprinted in A.G.N. Flew, *Logic and Language,* i. 117–144.

thereby committed to a metaphysical interpretation of ' soul '; no more does our rejection of phenomenalism entail that we must commit ourselves to some sort of Lockean substance.

Lucas also writes : " And if it really was *patterns* of behaviour and not the character of the behaviour that we were chiefly interested in . . . " (p. 142). It is implied here that the logical behaviourist is interested in ' patterns of behaviour ' but not in character. This, of course, is not so : the logical behaviourist *is* interested in character just as much as anybody else : where he does differ from other people is in his logical analysis of what is *meant* by character. Later (p. 142), reviewing his first two attacks on logical behaviour, Lucas writes : " These two grounds for hypostatisation are the same as those on which we defend our belief that material objects exist : we are *motivated* by a general nisus towards the greatest simplification of our thought and discourse and unification of our conceptual structure ; on these grounds we talk of persons as distinct from what they do in the same way as we talk of things and not merely how they appear " (our italics). Here, as in the first attack on logical behaviourism, the argument is vitiated by a confusion of logic and psychology. The logical behaviourist might, indeed, agree with Lucas's statement as an analysis of motives—he would not, however, agree that his theory is affected by it.

(iii) *Intuitive understanding* (pp. 142–144)

Two points here are immediately accepted : (*a*) that, as a matter of empirical fact, there is such a thing as intuitive understanding of other people (though some psychologists would certainly want to hold that such a trait is not very widely distributed) ; and (*b*) that Lucas's initial analysis of intuitive understanding is correct, *i.e.* that no use of an extra sense is involved. Where, however, we do begin to object is when Lucas rejects an analysis of intuitive insight in terms of unconscious inductions. There is a number of confusions here, which we shall try to elucidate. But, perhaps, at the basis of them all is, yet again, a confusion between saying something logical or epistemological and saying something psychological—an honourable enough confusion in view of Locke, Berkeley and Hume. (But not so respectable nowadays.) For Lucas seems to imply that, when he talks of " unconscious induction ", he is referring to some existential, though recondite, psychological process or happening ; thus, he says, for all the world as if he were discussing Freudian psychology, that " all explanations in terms of unconscious processes (are) hard to refute " (p. 143), and that such an explanation " does not accord with our own private experience " (*ibid.*). In fact, of course, nothing psychological is here being asserted ; rather, to talk of unconscious inductions is to make, or to begin to make, a logical analysis of statements which we call exercises of intuitive insight. Indeed, Lucas himself makes an analysis of them which is quite compatible with the unconscious induction type of analysis. For he says that those who are gifted

with intuitive insight " are marked by an ability to understand relatively much on a relatively small basis of observation " (p. 143), and they can do this because " they are possessed of a well of *singular hypotheticals* in themselves, so that given this or that expression, stance or behaviour, they can sense which of other actions, attitudes and responses would ' go with ' this one " (*ibid.* our italics). In other words (and we are here talking logic and not psychology), given a certain ' if ' clause, we have the ' licence ' (*cf.* Ryle) to predict a related ' then ' clause. All that is meant by saying that ' if—then ' propositions of this kind are unconscious inductions is that the evidence for (that is to say, the previous verified instances of the holding of) the relevant ' if—then ' relationships cannot be made explicit by the subject who uses it : he cannot specify the grounds of, or justify to another, his inference. It is simply on *this* account that we tend to talk of unconscious inference or unconscious induction : something is being said, not about the subject's psychological processes, but about the sort of evidence (or, in this instance, lack of evidence) the subject can give for certain of his statements.

Lucas's other arguments against unconscious inductions may also be viewed as instances of a confusion between the psychological and the logical. Thus, he says that " if intuitive understanding were just a matter of unconscious inference based upon sub-conscious memories, that is, if it were just a distillation of sense-experience, then it should increase proportionately with the increase of each individual's total sense-experience " (p. 143) ; but, says Lucas, it does not, or at least does not always do so. This would certainly seem to be true. What, however, we do doubt is whether a logical analysis of intuitive understanding in terms of unconscious inference entails that width of sense-experience is an important determinant of the phenomenon. For this, of course, is a psychological hypothesis—one suspects, an incorrect one : thus, the *notice* an individual takes of the actions and reactions of his fellow human beings may well be of critical importance here, and this is compatible with the emphasis of experimental psychologists upon the importance of ' set ' as a determinant of both perceptual and conceptual processes. But whatever the facts may be here does not matter for present purposes ; the fundamental confusion is still between the logical and the psychological, as indicated above. To specify the conditions of occurrence of intuitive understanding, on the one hand, and to analyse what sort of thing is *meant* by ' intuitive understanding ', on the other hand, are two very different types of activity.

Another argument Lucas uses against unconscious inference is " the evidence from great novelists and playwrights " (p. 144). Here Lucas cites two classes of alleged facts (for there are those who would question them) : (*a*) the psychological astuteness of some of the great novelists and playwrights, their intuitive understanding, and (*b*) how we often immediately feel that their insights are right. It is, however, by no means clear just why these facts, if they are facts, go against

the unconscious induction view of intuitive understanding. For (a) seems to be merely an extreme instance of intuitive insight, *i.e.* the sort of intuitive insight which is wide ranging and of rare occurrence ; but its logical analysis would be of the same sort as for more ordinary intuitive understanding—though, of course, it is possible that some of the great novelists and playwrights could make explicit the inductive bases of their insights. And (b), *prima facie*, seems more of a support than an embarrassment to the doctrine of unconscious inference : we knew what we had observed, but could not say what it was, could not make it explicit, and had to wait for someone else to say it for us.

Finally, we learn that " our normal ways of speaking . . . will not have it that our knowledge of other people is ' flat ' and all on a level with our knowledge of sticks and stones, but gives it a sense of depth and profundity ; a feeling that we can penetrate deeply into people . . . by our physical eyes indeed, but using them to observe acutely and actively, rather than waiting passively and dully for sense data to occur " (p. 144). Now, it may well be that acute and active observation is an essential precondition of intuitive understanding people ; it probably is. But, again, this is a psychological hypothesis, and as such is irrelevant to our logical analysis of intuitive understanding in terms of unconscious inductions ; for, as we have seen, to make such an analysis is merely to say that what is *meant* by ' intuitive understanding of people ' is, at least in part, propositions about people the inductive bases of which cannot be verbalised by those who make them. Once more, the essential confusion is that of supposing that talk about unconscious inferences is, in this context, talk about psychology.

But even if Lucas's analysis of intuitive understanding is correct—and this, be it noted, is essentially a negative analysis in terms of a rejection of unconscious inference—it may still be asked how does this go against the thesis of his main enemies, the logical behaviourists ? Lucas does not make the connection clear. Why is it that the existence of intuitive understanding (logically analysed *not* as unconscious inference, but not otherwise positively analysed) entails that we cannot analyse psychological terms as referring to behaviour or dispositions to behave ? And why, above all, does it help to establish the view that it is, after all, legitimate and useful to talk about the soul ?

(iv) *First-personal experience* (pp. 144–145)

" Our final reason for repudiating the reductive analysis of mind and spirit to patterns of behaviour is our own first-personal experience " (p. 144). We readily agree to Lucas's main thesis here : we all have experiences—pains, dreams, images, etc.—that may significantly be called ' private ' rather than ' public ' in the sense that we are in a privileged position for reporting them relative to other people. And it is not enough to analyse statements making references to such

experiences as meaning merely that certain patterns of behaviour occur or that there are dispositions to behave in certain ways in certain situations. Thus, in this sense, Lucas's repudiation of logical behaviourism is accepted : the proposed analysis does not enable us to deal without paradox with an important class of statement. But it is one thing to agree that logical behaviourism fails in this respect ; it is quite another to admit that it yields any consequences about either the legitimacy of, or the correct analysis of, ' the soul '. And this is just what Lucas seems to imply. For he says " ' Either I allow ' we say ' that other people have souls, or else I begin upon a systematic elimination of ' I ' " (p. 145). But, in *this* context, all that seems to be meant by saying of anyone that he has a soul is that he has private experiences ; and this very few would deny—not even Ryle, despite current misconceptions. Certainly, Lucas here gives no indication of what more might be meant. Perhaps, also, values enter in at this point, so that to talk of people having souls is to commit oneself to treating them with due consideration and sympathy—as human beings rather than as " automata " (p. 145). That ' soul ' may be analysed in either or both of these ways is plausible ; and if use of the term entails no more than this, then it may certainly be admitted that it is legitimate. But, in fact, we doubt whether Lucas's concept of the soul is as aseptic as this.

III. *Final Section* (pp. 145–148)

Here again we can go with Lucas some of the way : it is, indeed, " important to realise that the word ' soul ' does not have the same meaning in all its uses and contexts " (pp. 145–146). The two uses Lucas initially distinguishes are somewhat similar to those that we noticed immediately above, *i.e.* (*a*) as a word for imputing mind-like qualities to organisms, especially the capacity for having private experiences ; and (*b*) as a word which commits us to treating human beings according to certain moral conventions. As already indicated, these are both accepted. It is, however, worth noticing in passing the odd manner in which Lucas poses the problem of Other Minds : for him it almost seems to be some peculiar and very *recherché* empirical issue about the behaviour and experience of other people— whether or not they *are* automata. Surely it is more profitable to regard the problem as one about the kinds and standards of evidence (if any) we use for imputing states of mind to ourselves and others. Furthermore, whether experimental psychologists would agree to being forbidden to study private experiences, as Lucas seems to imply (see p. 146), is very doubtful. Certainly, this is just what many of them have been doing since the time of Galton (images) and Freud (dreams).

But it is when we come to Lucas's negative definition of the soul " by exhaustion " that we find our greatest difficulties. We are told of the concept of the soul that " we approach it by, as it were, a *via negativa*, always knowing and being able to specify what it is

not " (p. 146) ; we discover that the soul is something which *lies behind* the body, which is *something more* than the intellect, which is *beyond* personality, and which is also *beyond* morality. It is very difficult to know what sense to attach to such statements. Partly, this is the hypostatisation, already criticised ; partly, too, and relatedly, it is a classical example of a ghostly entity—a Rylean " ghost in the machine ". Lucas's *via negativa* in fact has led him back to the dualism which he seemed to reject at the beginning of his essay, where " the soul, although like part of the body, is itself imperceptible, immaterial, and non bodily " (p. 133). But, more important is that, in our view, Lucas here has gone *beyond*(!) logical analysis : he is talking in a mystical or religious way about human beings. Thus, he continues : " We yearn, not for a new law, but for a personal morality beyond the Law, beyond Pharisaism : for no other morality, however rational and however lofty, can ever really *get a grip upon the soul* ; only the *love of God* is adequate to win it, and perhaps only God is capable of loving one, not for some external attribute, but for oneself and soul alone " (p. 147 : our italics).

Illumination is temporarily cast upon one of the many senses of the word ' soul ' with which Lucas is trying to work when he suggests that " Often we can translate ' soul ' by *self* . . . " (p. 147). For, whatever the difficulties of making a logical analysis of ' the self ', to us they seem small in comparison with those involved in making an analysis of Lucas's ' soul ' (not interpreted in any of the senses so far suggested as legitimate). But almost immediately we find ourselves struggling with a sense of ' soul ' that is even more clearly metaphysical than that mentioned in the preceding paragraph ; for now it is " The essential substance underneath " (p. 147), the *quod substat accidentibus* of medieval philosophers ; whereas this concept is not necessary in respect of material things—or at least not in the medieval and Lockean senses, as we should surely all agree—it is not so clear, we are told, that we can do without it in respect of human beings. But it is nowhere indicated why this is so. Finally, we learn that an unequivocal definition of the soul, *i.e.* one which " exhausts the soul into a limited number of definitive qualities " (p.147), is sought only by those who " adopt the flat and superficial interpretation of the universe " (*ibid.*) But what counts as a " flat and superficial interpretation of the universe " is not made clear. Perhaps (see p. 148), it amounts merely to not believing in God. Perceptively, Lucas hints that " belief in God (is) a necessary and sufficient condition for belief in the soul " (p. 148). For the sense or senses of ' soul ' beyond those already admitted as legitimate, but which Lucas also seems to be trying to rehabilitate, *this* certainly seems to be true. But, then, just what is one committed to believing when one believes in God ? What is the analysis of ' there is a God ' ? These, indeed, *are* questions, interesting questions, but beyond the scope of this paper. It is not, however, so clear that they are beyond the scope of Lucas's ; for if belief in God and belief in the soul, in some

sense, are so closely connected, we are entitled to know what it is to believe in God.

IV. *Conclusions*

We would suggest, then, that Lucas has succeeded in his initial modest aim of showing that use of the term ' soul ' is permissible, though not obligatory.   For it has emerged from our analysis that there are at least four different senses in which ' soul ' may be used, and to which no objection need be taken by even the most tough-minded of philosophers.   These uses are :

(1) As a sort of *omnibus* way of imputing characteristically human qualities and attributes to organisms ; *e.g.* ' that dog positively has a soul ' (if it behaves very humanly), or ' he has no soul ' (if he behaves very inhumanly).

(2) Rather more specifically, as a way of imputing characteristically human private experiences to organisms ; thus, we tend to say of organisms which think, imagine, dream, feel and have aesthetic experiences that they have souls ; and of automata and electronic computers that they do not ; *e.g.* ' it can perform the most astonishing calculations, and even write love letters, but it has no soul '.

(It is evident that these two uses tend to run into one another.)

(3) As a way of committing ourselves to treating our fellow humans with due consideration and sympathy ; thus, in this usage of ' soul ', to say of people that they have souls is to say something evaluative— about how they should be treated ; *e.g.* ' remember, even your enemies have souls '.

(4) As a way of speaking about the self.

Other uses may be distinguished as follows :

(5) As a way of expressing our sympathy or pity for other human beings ; *e.g.* ' The poor soul ! ' or ' The wee soul is hungry '.   This might be called the emotive use of the word.

(6) As a way of indicating approval or disapproval of a person's moral qualities or aesthetic sensibilites ; *e.g.* ' he has no soul '—said of a reprobate (he robbed his mother) or of a philistine (he doesn't like music).

It is clear, then, that the word ' soul ' can be used significantly and sensibly.   But the road along which we cannot follow Lucas is the *via negativa* (*atque periculosa !*) of definition by negation and of hypostatisation.   Unfortunately, it is just here that the traditional and theologically important sense or senses of ' soul ' seen to emerge. It has not been our aim to deny that there can or may be such a sense or senses ; we have merely tried to show that understanding of them has not been furthered by Lucas's paper.

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In his recent assault on the autonomy of morals (MIND, July 1957) Professor Rynin maintains that moral statements are derivable from factual statements in the sense that some factual statements entail some moral statements. He maintains furthermore that some factual and some moral statements entail each other, but of this further claim I shall have nothing to say.

Rynin explains what he means by entailment as follows : " S entails S' ←→ [(S necessarily implies S') and neither S nor S' is analytic]" (p. 312), where " S necessarily implies S ' " means " (S.∼S ') is self-contradictory ". (I suspect that he actually intended to say, not " neither S nor S ' is analytic ", but " S is not self-contradictory and S ' is not analytic ", since he wishes to exclude the possibility of deriving any proposition from a self-contradictory one.) And he distinguishes between *factual* and *normative* statements as follows : factual statements are recognizable " by being verifiable or falsifiable, or possibly confirmable or disconfirmable, by reference to ascertainable matters of fact " (p. 308) ; normative statements " by the occurrence in them of certain terms, or their synonyms, or their antonyms, such as ' good ', ' right ', ' ought ', ' duty ', whose utterance is accompanied by feelings of moral approbation or disapprobation " (p. 308). He does not draw any distinction between normative statements and moral statements and, in fact, apparently treats the two terms, " moral statement " and " normative statement ", as interchangeable.

Rynin does not make clear, in his explication of " normative statement ", whether he means that there is a class of words whose utterance is, invariably or characteristically, accompanied by certain feelings and that every statement which contains one of these words is a normative one, or whether he means that a statement is normative if and only if it both contains one of a certain class of words and is accompanied by a certain feeling. As to the first interpretation, it is certainly not the case that when we use words like " good " and " ought " they are invariably accompanied by feelings of moral approbation or disapprobation ; nor, if " normative " means here, as it appears to do, " moral " (and if it does not it is difficult to see the relevance of the subsequent discussion of normative statements to the autonomy of morals), is every statement which contains one or other of the words listed by Rynin a normative statement. On the second interpretation a statement, to be classed as normative, must both contain a word of a certain class and be accompanied by certain feelings. And here the difficulty lies in the notion of accompaniment by a moral feeling—does " accompaniment " mean simply temporal coincidence, or does it mean that the feeling is in some way essential to the utterance ?

Rynin puts forward three arguments to prove that normative statements are derivable from factual ones ; the first two are designed to show that many people who explicitly deny his thesis nevertheless

assent to propositions from which it follows, the third represents Rynin's own view. The first of these arguments is as follows : Some people who deny that normative statements are derivable from factual ones admit that some factual statements are derivable from normative ones ; " many, in fact, hold that ' I ought ' entails ' I can' " (p. 308). But if N entails F then~F entails~N ; that is, if some normative statements entail factual ones then some factual statements entail normative ones. Rynin does not go into the matter any further but it ought to be noticed that the normative statements represented by N are not statements to the effect that something ought or ought not to be done but are statements to the effect that it is not the case that something ought to be done. They might be called " permissive " as opposed to " prescriptive " normative statements. It is doubtful whether such permissive statements satisfy Rynin's definition of " normative " (in fact he subsequently (p. 316) denies that statements which grant permission to do something are functionally statements at all) ; however, by any ordinary standards they must be classed as moral statements.

There is, however, a serious objection to Rynin's first argument. The familiar maxim is " ought *implies* can ", and this and the convention of discourse which it summarizes would appear to be correctly interpreted to mean, not " ought *entails* can ", but " ought *presupposes* can ". [I am indebted to Dr. D. G. Brown for this point.] That is, if I say " You ought to swim out and rescue him " I do so on the presupposition that you can swim. If you cannot my remark is not false but inappropriate—if you cannot swim then the question of whether you ought or ought not or may choose whether or not to swim simply does not arise. " I cannot swim " does not entail " I may refrain from swimming ", since I can only refrain from swimming if I am able to swim.

Rynin's second argument is this : Some philosophers, in dealing with arguments like " You promised to go. Therefore you ought to go ", have denied that the first statement by itself entails the second and have maintained that the argument is an enthymeme involving the suppressed premiss " One ought to do what one has promised "—a premiss not itself derivable from any factual ground. We then have a valid argument of the form : [(F.N)e-→N '], where " e→ " stands for " entails ". But, in accordance with the principle of exportation this gives us : [Fe→(N→N ')]—that is, " You promised to go " entails " If one ought to keep one's promises then you ought to go ". And here, according to Rynin, we have a case in which a normative statement has been derived from a factual one.

One comment should be made with regard to the logical transformation employed in the foregoing argument. This is said to be in accordance with the principle of exportation—that is, the principle that any expression of the form " p and q materially imply r " is logically equivalent to an expression of the form " p materially implies that q materially implies r ". In fact, however, the transformation in question is in accordance with a different principle—the principle

that any expression of the form "p and q entails r" is logically equiva-
lent with an expression of the form "p entails that q materially
implies r". And that this principle is not itself logically equivalent
to the principle of exportation can be shown by showing that it is
false. Suppose that B by itself entails C, then, given a statement A
logically irrelevant to B and C, (A.B) entails C. Now by Rynin's
principle A entails (B→C) ; however this is not so in the case under
consideration since (B→C) is analytic, and Rynin has specifically
ruled, in his definition of entailment, that if S entails S' then S'
cannot be analytic.

However, if we accept Rynin's modified principle of exportation as
holding, as it appears to do, for arguments in which p and q together
entail r but neither p nor q by itself does so, then we may still ask
whether a proposition of the form (N→N') is really normative.
Rynin's reasons for believing that it is are as follows : If (N→N') is
not normative then neither is (F→N') ; but if (F→N') is factual
then, if we go back to the argument [(F.N) e→N'], we can transform
it into [Ne→(F→N')], which can in turn be transformed into [∼(F→
N')e→∼N]. And again we have a case in which a factual statement
entails a normative one. As for the possible suggestion that (F→N)
and (N→N') are neither factual nor normative Rynin confesses him-
self here beyond his depth.

The foregoing reasoning, although ingenious, is weak in at least
two respects. First, with what justification does Rynin assume that
(N→N') and (F→N') belong to the same category ? In the argu-
ment under discussion these expressions represent :

   (i) If one ought to keep one's promises then you ought to go.
   (ii) If you promised to go then you ought to go.

Now (ii) clearly expresses a moral judgment, whereas (i), although
such a form of words *could* be used sarcastically to express a moral
judgment, does not in its most natural employment, and in the
employment dictated by its present context, express a moral judg-
ment but explicitly reserves judgment. Second, Rynin presumably
intends to indicate that he finds unintelligible the suggestion that
there are statements which are neither factual nor normative as he
has defined these terms. If this is a correct interpretation of his
remarks it is not at all clear why he thinks this. In which of his
categories would he place " If A is taller than B then B is shorter
than A " or " You ought to see Marilyn Monroe in ' The Sleeping
Prince ' ", assuming the latter statement to be accompanied by a
non-moral feeling of approbation ? It seems, in fact, unquestionable
that there are other categories than factual and normative, and that
(i) belongs to one of these other categories.

Rynin's first two arguments for proving that normative statements
are derivable from factual ones are based upon views held by other
people but not necessarily by himself ; his third argument represents
his own view. It consists in denying that such an argument as " You
promised to go. Therefore you ought to go " is an enthymeme and
in maintaining that it is itself a complete valid argument in accord-

ance with the principle of inference " One ought to do what one has promised ".   Such a principle of inference is not itself an unstated premiss, nor is it a conclusion from other premisses—" We arrive at a rule of inference by becoming aware of the fact that we understand certain statements in such a manner that if the one is true the other must be true . . ." (p. 316).

Rynin's argument is that " You promised " entails " You ought " and consequently that there is at least one type of case in which normative statements are derivable from factual ones.   But how do we become aware that one statement entails another ?   Rynin rejects the old formula that the conclusion of a valid argument contains nothing which is not already contained in the premisses. In its place he employs the notions of necessary and sufficient truth conditions—we become aware that one statement entails another by ascertaining that an " ntc " for the first statement is an " stc " for the second.   However, what this comes to, in terms of Rynin's definitions of " ntc " and " stc ", is that a statement A can be seen to entail a statement B by ascertaining that there is a statement C such that A entails C and C entails B.   And this, if not viciously regressive, is at any rate quite unhelpful as a recipe for detecting entailments.

Now it will be recalled that as Rynin defines the concept of entailment, to say that A entails B is to say that $(A.\sim B)$ is self-contradictory although neither A nor B is by itself self-contradictory.   The old-fashioned view of entailment was that the conclusion of a valid argument simply made explicit what was contained, implicitly or explicitly, in the premisses.   This does not mean, as Rynin believes, that no terms can appear in the conclusion which have not been employed in the premisses, but only that, given a valid argument, if we fully set out what the premisses say, by explicating the concepts employed in them and by revealing all the relations asserted between these concepts, we shall find that we have in the process set out what is said in the conclusion.   Now although Rynin rejects this, undoubtedly imprecise, analysis of entailment it is nevertheless toward something very much along these lines that his own definition points.   If I contradict what someone says I do so by denying his statement—that is, I say in effect " It is not the case that x ", where x stands for what he has said or for part of what he has said.   Similarly if the conjunction of A and B is self-contradictory, although neither A nor B is by itself self-contradictory, then B must represent the denial either of the whole or of some part of the statement represented by A.   Hence if A entails B then B must either say what A says or it must say some part of what A says.   But since factual statements say simply how things are, whereas normative statements direct us to act in certain ways or express commendation or condemnation, and since these represent entirely different linguistic activities, many philosophers have concluded that no factual statement by itself entails a normative statement.

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IN REPLY TO HART AND HAMPSHIRE

In their recent article in MIND,[1] Hart and Hampshire say : (1) "There is a kind of certainty about human actions, wants, likes and dislikes, which is different from the kind of certainty about these subjects that is based upon empirical evidence : it is a kind of certainty, or knowledge, to which the notion of evidence is irrelevant." Further, in discussing "a man's knowledge of his own present and future voluntary actions" they say : (2) "Our thesis is that there is a necessary connection between certainty of this kind, and on this topic, and deciding to do something. . . ." At a later point, in discussing decision specifically, they claim : (3) "It is clear that a person's announcement of his intention to do some action in the future is not a prediction that he will do this action, although others may base their predictions upon such announcements made by the agent. That such statements are not predictions is evident from the fact that if the agent does not act as he says that he intends to act, this exposes him, not to the criticism that what he said was false, or that he was mistaken, but to the charge that he has changed his mind "—assuming that he was not lying about his intentions. They say furthermore : (4a) that if a man says " I have decided to do X ", this entails : " I am certain that I shall do X, unless prevented, or unless I fail by reason of circumstances outside my control ", where the " certainty " mentioned is stated to be " non-inductive " certainty ; (4b) : the minimum force of " I intend to do X " is : " I believe that I will try to do X if the occasion arises." Hart and Hampshire finally, after pointing out (5) the asymmetrical nature of 1st- and 3rd-person decision-statements and inferences as to future actions which may be drawn from them, admitting that any " certainty " we have about future events which is based on 3rd-person statements is purely inductive certainty, state categorically that a 1st-person decision to do something affords non-inductive (and, by implication, stronger), certainty that the action will be performed than 3rd-person, or inductive, certainty does. The purpose of this note is to point out that there is a fundamental ambiguity in the article concerning the sense in which Hart and Hampshire are using " certainty ".

We commonly distinguish three kinds of certainty : (i) mathematical, (ii) logical, and (iii) scientific (causal or statistical) certainty. Now by " practical certainty " Hart and Hampshire certainly do not mean (i) or (ii) and deny that they mean (iii) (see quotation 1). The question therefore arises in what sense they are using the word " certainty ". There is a possibility that they may be using the word in a new sense, but if so, it is not clear what this sense is. They do not claim to be using it in a new way : what they seem to be doing is to use it in two different established ways, which are in fact

incompatible, in that one is inductive and the other non-inductive. The first of these (see quotation 2) is the philosophical, absolute sense (where they assume the incorrigibility of 1st-person statements about consciousness) ; the second is the sense in which " certainty " is equivalent to "certitude ", defined by the Oxford English Dictionary as " subjective certainty ". Now certitude, when it concerns our future voluntary actions, is normally based partly on direct and incorrigible knowledge of the "absolute " feeling of certainty of decision, and partly on inductive evidence. Hart and Hampshire do not give an analysis of their term " voluntary act "; if it is taken to mean the goal, or final action envisaged, then any certainty (certitude) we have about this act will be largely inductive, and decision-statements referring to such acts will be predictions. Hart and Hampshire seem to be using the term in this sense, and yet to be trying to have some of the advantages of a different analysis, one which is not in fact available to them. This second analysis is the one in which " voluntary act " is taken to mean the momentary " act of free-will ", with regard to which we can have certitude amounting to absolute certainty, just as we can have such certitude that we have made a decision (this certitude is the basis for the distinction drawn by Hart and Hampshire between 1st- and 3rd-person decision-statements). But certitude of this type is not confined to decision-statements : it is common to all honest 1st-person experiential statements. Furthermore, even though there be a non-inductive premiss (*i.e.* that I have made a decision to do X) in the argument whose conclusion is " It is certain that I will do X ", the suppressed premisses are inductive statements concerning the laws of nature, including psychological laws ; the conclusion itself is therefore inductive. Hart and Hampshire may admit that the conclusion is not purely non-inductive, but may claim that we have good non-inductive grounds for our certitude about the conclusion. But even if such grounds can be found, they do not make the certainty of the conclusion : " It is certain that I will do X " non-inductive, and in so far as they are predictions, such statements will be open to the familiar Humeian objections.

Before discussing the way in which Hart and Hampshire try to avoid such objections, it is interesting to ask why they should want to find grounds for denying the relevance of empirical evidence to decision-statements. Upholders of a free-will doctrine, rather than determinists, would, I think, want to claim a special sort of certainty in relation to decision-statements, as Hart and Hampshire are doing. They might not be prepared to say that we have free-will at all times, but would at least claim that there are many times when we can, by a free choice, direct our actions as we decide. Now if free-will is to be claimed at all, it is necessary that, at times when we exercise our free choice, we should do so consciously, and that we should have direct and incorrigible knowledge of what we are doing, in order that we may be held fully responsible for our voluntary acts. But in

17

order to claim this incorrigible knowledge, and also to claim absolute
certainty that our " act of will " will be followed by the appropriate
voluntary act, a free-will theorist will have to distinguish between
the voluntary instigation of a chain of events likely to lead to the
desired result, X, and the later part of the chain, which is controlled
causally. It is only over the first step of the chain that we can have
control. The weaker form (4b) which Hart and Hampshire suggest
has affinities with this free-will theory, in which the " voluntary
act " is reduced to one step ; thus " I will try to do X " could be
interpreted as (4c) : " I will set in action the causal chain (or chains)
most likely to lead to X, and I will make an attempt to overcome
obstacles, sub-conscious or external, which would prevent me from
doing X ". On this interpretation, according to which the " voluntary
act " is to *try* to achieve the required result, it is perfectly legitimate
to say that a paralysed man is performing a voluntary action if he
is genuinely trying to move his leg, even though, in fact, no movement
of his leg takes place. " Trying " may be regarded as a mental state,
of which we can have 1st-person, incorrigible knowledge, and about
which we can make true statements which are not predictions ;
but a man's knowledge that he is trying to do X is not the same as
his knowledge that he will achieve X. Even the statement (4c),
which is only a part of what we normally mean when we say :
" I have decided to do X ", could be known with absolute, non-
inductive certainty only at the moment of doing X, since any time-in-
terval admits the possibility that I may change my mind. (It is not the
case (see quotation 3) that, every time a person fails to carry out an
action which he had decided to do (or to try to do), the explanation is
that he has changed his mind ; he may simply forget, or be prevented
due to external conditions or sub-conscious resistances.) However,
absolute certainty of such a limited kind is of little practical interest
to us, mainly because, when we speak of " voluntary acts ", we refer
not only to the chain of events leading to X, but also to X itself.
This is shown by the fact that a person may decide to do X, fail to
achieve X, and yet claim, in retrospect, that he had done what he had
decided to do : but in this case he will say, not " I decided to do X ",
but " I decided to try to do X, and I did ". Of course, we do limit
the range of the causal chain we are prepared to regard as included
within the act : unforseeable consequences are not so included,
and the agent is not regarded as culpable with reference to such
consequences. We even vary the range of the act according to the
nature of the agent : a child is not held responsible for as many
consequences of his free-choice as is an adult. But we do not, in
practice, limit the process to one step.

Moreover, our decision-statements are further complicated in that
reference to our future voluntary acts as described above is only a part
of what we usually mean when we say : " I have decided to do X ",
and on a determinist view, is never part of our meaning. We generally
imply that we think it likely that the obstacles will in fact be overcome.

This is not clear from Hart and Hampshire's analysis of " I have decided to do X "; they try to avoid Humeian objections by adding qualifications to the statement "It is certain that I will do X " (see quotation 4a) : and later weaken 4a still further, to 4b. But 4a is equivalent to the tautology : It is not the case both that I shall do X, and that I shall be prevented from doing X. Surely, the only ways in which 4a says anything meaningful are : firstly, that when used in ordinary speech, the speaker implies that it is unlikely that he will be prevented—this is an inductive matter. Secondly, he implies that he does not wish to be prevented (if we say he *entails* this, we get into difficulties over remarks such as : " Well, all right, I'll do it ; but I don't really want to "). On Hart and Hampshire's analysis, it would be quite in order for a sane man to say : " I have decided to walk through the sun ", in spite of the fact that he would readily admit that he was hardly likely to succeed. In ordinary language, such a decision-statement would not be accepted as a serious remark if made by a sane adult, nor would the speaker himself be able to take it seriously. If a chain-smoker says, regularly, " I have decided to give up smoking ", and never does, then—on inductive grounds—we cease to infer from his remark that he will in fact give it up. Even the speaker himself may not infer from his remark that he is likely to stop smoking, if he has already found from experience that the habit has a very great hold on him. If he does infer this, it will be because, as well as having made the decision, he has good inductive grounds for believing that he may succeed. It is because most of our decision-statements are based on inductive expectations of this kind, that it is misleading to say " I have decided to do X " when one does not really think it likely that one will succeed. And this is one reason why 1st-person decision-statements may seem to involve a superior kind of certainty about the occurrence of the envisaged act, when compared with 3rd-person decision-statements ; but the greater probability involved in the former statements is a probability based on inductive beliefs. The second reason is that only the speaker can be quite sure that he is not lying ; this non-inductive certainty is not peculiar to decision-statements (although it is of special importance to anyone upholding a theory of free-will), and is apt to be confused with the certainty (certitude) one feels due to one's faith in the inductive principles involved in the realization of the required result.

Thus there is no intermediate kind of certainty, between absolute and inductive certainty, and superior to the latter, associated with 1st-person decision-statements. There is merely subjective certainty, or certitude, that there is a high probability that the agent will successfully achieve the intended act. Such certitude is composed of two elements : one, the incorrigible certainty common to all 1st-person experiential statements, but of special importance to opponents of determinism, and the other, inductive certainty, which is usually believed to be of a high probability in seriously-made decision-statements. This mental state of certitude may lead us to

say that " I have decided to do X " entails " It is certain that I will do X "; but the certainty involved is largely inductive unless it is made of such a limited range (*i.e.* as referring to the momonetary " act of free-will ", the trying to do X) as to lose application in ordinary speech. For, in ordinary speech, the verb " do " usually means " achieve ", rather than " try to achieve ", and the required result, X, is included within the meaning of the term " voluntary act ".

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X.—CRITICAL NOTICES

Intention. By G. E. M. ANSCOMBE. Oxford : Basil Blackwell, 1957, pp. ix, 93. 10s. 6d.

THE interest taken by most philosophers in intention and allied concepts tends to be peripheral. Much has been written on them from an ethical point of view. But even there, more often than not, they have finally been consigned to the care of moral psychology with a lack of regard more appropriate to pulling the waste plug. They have figured as peripherally in recent demonstrations of the need for a more flexible understanding of language. This book belongs to the latter rather than the former genre but is peculiarly welcome for its undistracted preoccupation with its subject. The authoritative and original way in which it resumes certain enquiries left tantalisingly short by Wittgenstein adds to its interest.

The term ' intentional ' does not refer to a natural class of event distinguishable by something extra or internal but ' to a *form* of description of events ' (p. 84). Miss Anscombe supports this thesis by defining intentional action as action to which the question ' Why ? ' in a certain sense has an application (p. 9).

The following sections give interlocking accounts of (*a*) the various kinds of answer which allow the question application in this sense and (*b*) the conditions under which it is inapplicable. (*a*) The answers neither give evidence nor causal explanations, but either recount past history viewed as good or harmful, or provide enlarged descriptions of the actions, or predict future events. Less direct answers such as ' No particular reason ' or ' I don't know why ' give application to the question as well. (*b*) To determine the conditions of inapplicability without circular appeal to the notion of intention those of knowledge without observation and mental causality are introduced. Briefly, the question is inapplicable if the agent is unaware of the action, or if he knows of it only through *observing* what is happening, or if there is no room for mental causality, *i.e.* if he has to speculate about what caused his behaviour.

The mutual cohesion of these two accounts is not entirely plain to me mainly through my not understanding the underlying distinction between the sense of the question and the conditions of its inapplicability. However, not much weight is placed on (*b*) in later sections, and so I pass on to these without further comment. There the notion of mental causality plays no further part, and instead of the question ' Why ? ' being elucidated by the companion notion of knowledge without observation, the *practical* form of the latter is elucidated in terms of the question ' Why ? '

In intentional action we don't need to find out what we are doing before we know what we are doing. But we should not infer from this, as some may, that two objects become known to the agent, *viz.* the intention, known without observation, and the physical results which we observe. Practical knowledge is not concerned

with the correspondence between belief and any kind of fact. Its
concern, Miss Anscombe thinks, is with the correctness of the *action*.
When what I believe I am doing intentionally does not correspond
with the fact, the mistake lies in the action not the description.
Consequently, to elucidate practical knowledge, she turns to practical
reasoning, the outcome of which is action, not conclusions.

Practical reasoning becomes possible where certain series of descrip-
tions apply to the same intentional action. These give answers to the
question ' Why ? ' where the later members depend upon a wider set
of circumstances than the earlier. Thus where ' moving my hand
up and down ', ' pumping ', ' replenishing the house water supply '
and ' poisoning the occupants ' are all descriptions of the same action,
the following dialogue becomes possible : ' Why are you moving,
etc ? ', ' Because I am pumping ', ' Why are you pumping ? ',
' Because I am replenishing, etc.' ' Why are you replenishing, etc ? '
' Because I am poisoning, etc.' Now a series of this sort also forms
the basis for an Aristotelian practical syllogism. In reverse order
the descriptions specified by the answers now become descriptions of
desirable conditions functioning as premisses which should issue in
actions fitting the description specified by the questions. This is
how practical knowledge viewed as practical reasoning involves error
in action rather than judgment.

This elucidation of intentional action via practical knowledge,
practical reasoning, and finally the question ' Why ? ' leads one to
wonder why certain things should be subject to this question. But
Miss Anscombe rejects this speculation on the ground that the
description of a movement as a human action is no more prior to the
question ' Why ? ' than the description of marks as a sentence is
prior to the question ' What does it say ? '. And this is the cue for
her central thesis that ' intentional ' refers to a *form* of description.

When a cat slinks along with a fixed gaze in stalking a bird, it is
' stalking ' not merely by doing this in that but also because its idea
of doing this in that is a necessary condition of the performance.
For these reasons Miss Anscombe follows Aquinas in defining practical
knowledge as ' the cause of what it understands ', but insists that it
' can seem an *extra* feature of events whose description would other-
wise be the same only if we concentrate on small sections of action
and slips which can occur in them ' (p. 87). *I.e.* descriptions like
' stalking ' can rightly apply only to continuously supervised action.

This argument, however, fails entirely to show that ' intentional '
refers to no extra feature of events *period*, which is the issue at stake.
It may establish that ' intentional ' refers to a form of description,
but only because this formula is more enigmatic than Miss Anscombe
thinks.

The primary flaw in ' Intention ', however, is that even if practical
reasoning must involve practical knowledge the converse need not
hold. Practical knowledge defined as the cause of what it understands
is a detachable notion which suffices for a definition of intentional
action without any resort to the question ' Why ? '

'Why ?' is an enquiry into the motive for doing an action, or, in Miss Anscombe's terminology (p. 9), into a person's intention *in* rather than his intention *of* doing something. Now when we say that an action is intentional our point is that the agent had the intention *of* doing the action as we have described it. We are not necessarily pointing to any motive behind it. The distinction I have in mind can be defined as follows. The intention of an intentional action is anything which the agent thinks will come about, or is coming about, through his action. The motive consists only of that for the sake of which it is done. Dr. Paccard's motive in making the first ascent of Mont Blanc has been described as scientific rather than athletic. His intention presumably was both along with other things.

Again, intention is more basic than motive. In asking why somebody did action AB we are speculating (if the action is over) whether he would have intended to do A rather than B if the situation had compelled him to choose between them. If he would have done A rather than B, Miss Anscombe thinks (p. 89) that his actual doing B along with A is not intentional, and downright involuntary if he regrets doing B. It is more accurate to say that relative to the situation it is perfectly intentional. What *is* involuntary is his having to choose in the situation in which B has to go with A, if, given the choice, he would have avoided this situation in favour of another in which A and B are incompatible.

It follows that *expressions* of intention have greater significance than Miss Anscombe allows in the opening sections of her argument. With most of what she has to say about them I am in complete accord. They *are* predictions of a queer kind, but their queerness does not directly consist in their being backed by answers to her question 'Why ?' instead of by evidence. On the contrary, it matches the queerness of practical knowledge as the cause of what it understands : and this is why a definition of intentional action in terms of practical knowledge is superior to a definition in terms of practical reasoning. An expression of intention ' I am going to stay here ' is a queer prediction because unless the speaker's awareness of what he is about to do is a necessary condition of its coming about, his action is a mere happening.

Note as well that there is no simple identity between this causal type of understanding and the process of supervision involved by practical reasoning. In certain descriptions of practical reasoning such as ' stalking ' or ' telephoning ' the description of the mere physical activity is indeed inseparable from the description of the activity of supervision. This is mainly because they are descriptions of fairly complex processes and the complexity of the process bears witness to their intentionality. But what makes the intentionality of actions most evident to others need not be its most essential feature.

The approach through the question ' Why ? ' has a further disadvantage. It obscures the crucial distinction between the first person singular and other persons. It is neutral in this respect, but practical knowledge isn't. I can apply the question to your

actions but exercise practical knowledge only in my own. Hence if practical reasoning consists in understanding the connections between descriptions into which the question ' Why ? ' enquires, it must consist of theoretical understanding, and its mistakes are primarily theoretical mistakes, even where they have practical consequences. When we turn to expressions of intention, on the other hand, the importance of distinguishing first person singular from other forms of speech is evident. ' I am about to do X ' can be an expression of intention, ' He is about to do X ' normally is not, though it may be a prediction based upon the intention which the former expresses.

This finally brings me to Miss Anscombe's bugaboo, viz. intention as some internal extra. To define practical knowledge as the cause of what it understands and then to deny that it is in some way additional to what it understands must nullify the explanatory value of the definition. But this definition is after all deficient. It only works in the second or third person singular or in the past tense. My prediction ' He is about to do X ' of A's intentional action is based upon my discerning his intention *and* the standing conditions of the action. In my prediction of his action the two are external to each other. To this extent intention must be an extra. In expressing my own intention, however, one cannot without reflexivity and consequent regresses say that I base my prediction upon the fact of my intention *and* the standing conditions. The prediction is the vehicle of my intention and is made in consideration of the standing conditions. The two are not external to each other. This is the root of the distinction between the two kinds of prediction. What I intend cannot enter into my prediction, whereas what other people intend can, since they may belong to the standing conditions which I have to take into consideration.

The alternative to the formula of Aquinas is a clearer understanding of the relation between my present and my future which is involved in my action. From my point of view my present is not a state which mediates temporally or causally between my past and my future states. It is simply the state of having at the moment a certain past and being about to have a certain future. The distinction then between practical and theoretical knowledge is that the latter comprehends only predictions of the sort which should have a sufficient basis in what has happened up to the present moment, and this includes other people's intentions. Practical knowledge on the other hand comprehends predictions which cannot have a sufficient basis in what has happened up to the present, simply because the intention expressed by these predictions cannot enter into the calculation. This opens the door, of course, to the subject of free will, but it is an incomplete sort of enquiry into intention that does not.

My fear in concluding is that I have done less than justice to the insight which Miss Anscombe shows throughout her enquiry, by linking her argument too closely with a central theme which leaves me unconvinced.

                                                    K. W. RANKIN

Laws and Explanation in History. By WILLIAM DRAY. Oxford
University Press, 1957. Pp. 172. 21s.

MR. DRAY's primary purpose is to refute a certain view of the nature
of explanation in history. This is the view that explaining a his-
torical event consists in citing independently established particular
conditions and either citing, or tacitly appealing to, independently
established laws such that the occurrence, or the probability of the
occurrence, of an event such as that to be explained follows analyti-
cally from the statement of these conditions and laws. He calls
this view ' the covering law theory ' of historical explanation. He
argues, first, that it is not a necessary condition of explaining an
historical event or complex of events that an appeal, even an implicit
one, should be made to a covering law (ch. 2) ; and, second, that
subsuming an event under a law is not a sufficient condition of
explaining it, either in history or elsewhere (ch. 3). Next he argues
that both these contentions hold good even if we restrict the class
of general laws to what may reasonably be called causal laws ; and
he follows this up with a discussion of the historian's use of the word
' cause ' (ch. 4). Finally, he discusses two types of historical
explanation to which he thinks the covering law theory is peculiarly
ill-adapted : *viz.* explanations in terms of individual agents' reasons
for acting as they did (ch. 5), and explanations which show how
something was possible rather than why something was inevitable
or to be expected (ch. 6).

Mr Dray's book is a good one. It is written with care, modesty
and, at times, a certain elegance. Mr. Dray demonstrates in his
first chapter that many philosophers have thought of historians as
operating just the same kind of logical apparatus as natural scientists,
the difference being one of location of interest : the historians' general
laws are vague and boring, while their particular instances are
interesting ; the natural scientist has no interest in particular cases
as such, but only in the precise and relatively complex general
theories which those cases enable him to test and, sometimes, to
confirm. This picture of the relations of history and natural science
may appeal by reason of its neatness. Mr Dray shows successfully,
in the chapters which follow the first, that it travesties the character
of historical explanation. He is, I think, somewhat over-concerned
to stress the enormity of his opponents' misunderstandings. Some-
times this over-concern leads him to stop short at crucial points in
his own positive account, willing rather to leave us with a mystery
than to uncover grains of truth in the disputed thesis. Sometimes
it leads him to flirt uneasily with dubious doctrine.

In chapter 2 he considers the view that an appeal to law is *implicit*
in any historical explanation. If so, it should be possible, in any case
where the historian presents certain facts as explaining others,
to discover *what* law is in question. But must the historian regard
himself as committed to *any* law, or laws, which, taken together

with the explaining facts, entail a statement of any facts of the
general character of those to be explained ? Perhaps, when the
historian's account reaches a certain degree of fullness of detail, we
may get him to agree that he is committed to assenting to what is
formally a general statement : a statement to the effect that in any
set of circumstances just like those described in the full explanation,
events just like those to be explained would occur. But this is the
mere form of generality, and the idea that such a 'law' could be
established independently of the particular case concerned is absurd.
Mr. Dray argues this point admirably. He makes also the comple-
mentary point that it is no use defending the theory of the covering
law by appealing to the fact that historians use classificatory terms,
like 'war' and 'revolution', which can themselves figure in
generalisations ; for the historian's explanation of the French
Revolution is in no sense an application of a general law about
revolutions. The historian explains that particular course of events
which we refer to as the French Revolution, and to do this is not to
apply a theory of revolutions in general. On the one hand, then,
we may have the mere form of a highly specific 'law' ; on the other,
we may have the substance of a highly general generalisation. But
neither can be regarded as invoked by the historian's particular
explanations ; rather, both presuppose those explanations.

So far, so good. The doctrine that there must be independently
established laws which really cover the cases, in a tight logical sense,
is shown to be unrealistic. But what of Mr. Dray's positive account ?
Are we to conclude that generality has no place in particular his-
torical explanations, that there is no sense, for example, in which
such explanations invoke platitudes about how human beings may
be expected, individually or collectively, to behave in certain
circumstances ? Mr. Dray seems at times to encourage us to think
so. He constantly refers to the historian's exercise of *judgment*,
almost as if this faculty were a substitute for any such invocation.
The historian *judges* that such-and-such an event was inevitable in
such-and-such circumstances (p. 55) ; he *judges* in a particular case
that there is a necessary connection between the event and the
circumstances cited to explain it (p. 158) ; he appeals to his critics'
judgment that such-and-such a state of affairs would result from
certain conditions (p. 51). But what makes such judgments accept-
able ? How is it that judgments coincide as much as they do?
Surely something about general knowledge of human nature, about
human experience of how things tend to go in the worlds of private
and public affairs, might be mentioned here, not indeed by way of
providing covering laws, but in the course of explaining what the
exercise of judgment is, and how it can establish these satisfactory
connections of which Mr. Dray speaks. As it is, 'judgment'
appears as a slightly mysterious gift of nature to historians.

I think there is a twofold explanation of Mr. Dray's reticence on
this point. Partly it is the result of that over-concern with his

opponents' folly, which will not allow him to grant them their grain of truth. Partly it is the result of an assumption which he seems to share with his opponents : *viz.* that a satisfactory answer to an historical ' Why ? ' question must show that, ' in the light of certain considerations ', the event about which the question is asked is one which ' had to happen ' (p. 161) or was *inevitable* (p. 55), that between the event and the circumstances cited to explain it there was a *necessary connection* (p. 158). Now it is indeed true that, in the typical historical explanation, knowledge of the particular circumstances cited as explanatory, together with knowledge of general truisms about human behaviour, do not yield the conclusion that the event to be explained *had* to happen. If it is the function of the faculty of judgment to discern this necessity, no wonder Mr. Dray is a bit reticent about this faculty. But surely no such discernment of necessity is required in historical explanation. There is no doubt about the *occurrence* of what is to be explained ; and explaining it is not a matter of showing that it *had* to happen as it did, but that it is entirely natural that it should have happened as it did. To achieve this satisfactory result does not seem to require the exercise of any special faculty for discerning necessary connections between particulars. Of course it requires more or less detailed knowledge of the circumstances surrounding what is to be explained, and knowledge, too, of the wider historical setting. But are no analogies with other cases, no truisms about human behaviour, at work in guiding the selection of facts and their arrangement into explanations which satisfy the judgment ?

I should say that there are passages in Mr. Dray's book which seem to indicate that he would not wish to dispute what I am here suggesting. In another chapter, for example, he cites with approval writers who connect the concept of explanation with that of familiarity ; and in chapter 2 itself there is one paragraph (on p. 55) which, though I am not entirely sure about its interpretation, seems to be in the spirit of the suggestions I am making. But I wish he had been less reticent about judgment.

Chapters 3 and 4 contain interesting and valuable discussions of the notions of explanation and of cause. Mr. Dray makes many good points, though some of his conclusions seem debatable. These chapters are not crucial for his general thesis, as chapter 2 is, and I shall not discuss them in detail. In chapter 5 Mr. Dray concentrates attention on those cases in which the historian seeks to explain particular actions of particular historical personages who play an important role in the story he is telling. To these cases, he thinks, the ' model of the covering law ' is peculiarly inappropriate. I think he is right about that ; but his reasoning in this chapter is elusive, and I suspect that he is failing to distinguish two theses, one correct, the other highly dubious. Of course in order to understand why a person acted as he did, we must often be able, as Mr. Dray puts it, to reconstruct his calculation, to appreciate the situation

from his point of view, to see what reasons were decisive for him. But Mr. Dray seems not to distinguish between reconstructing a calculation and endorsing it as a correct calculation, between seeing what reasons were decisive for an agent and seeing those reasons as, from the agent's point of view, decisive reasons. He seems to think that while it is possible, consistently with understanding an agent's performance, to attribute to him defective moral principles or defective information, it is impossible to attribute to him defective judgment. It is not enough, it appears, to see that, given the agent's beliefs, principles and purposes, the thing he did would seem *to him* the thing to do ; it is necessary to ' see ' that, given those beliefs, principles and purposes, the thing he did *was* the thing to do. Mr. Dray condemns anything which stops short of this as *spectatorism* in history ; I should say that anything which goes as far as this makes history impossible. For the historian has to reckon with the fact that different calculations are possible in the same situation, and that these differences are not traceable only to differences in beliefs, purposes or moral principles ; they may be traceable also to differences in intelligence and temperament, ability and character. The historian, like the student of contemporary politics or anyone who seeks to understand the behaviour of his friends, enemies or colleagues, must know his man ; and this is certainly something different from either knowledge of platitudes about men in general or acquaintance with psychological theories. So the covering law model is indeed peculiarly inapplicable here. But knowing your man is not *sharing* his powers or character, any more than it is sharing his principles or purposes or beliefs : it is not *sharing* his courage, his optimism, his complacency or his stupidity. It is true, I think, that in order fully to understand, we must, on our own account, know something of what it is like to *be* complacent or stupid or vain or humble or even courageous ; and any human being is likely to know this. Perhaps this is all that Mr. Dray means. But I do not think it is, all the time.

The point made in Mr. Dray's last chapter does not give such powerful support to his main thesis as he seems to think. He says that sometimes what is required by way of explaining something is the ability to point to some fact which successfully counters a prior presumption that the thing to be explained was impossible. But this is a pretty neutral point as between him and his opponents. For thus to counter a prior presumption against something's being possible *might* simply be to show that it was not, after all, an exception to a law ; and even the most rigorous covering law theorist would probably be willing to qualify his account of explanation sufficiently to admit this case, without feeling he had thereby surrendered any substantial part of his position.

<div align="right">P. F. Strawson</div>

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XI.—NEW BOOKS

Socrates, Man and Myth: the Two Socratic Apologies of Xenophon. By
ANTON-HERMANN CHROUST. Routledge and Kegan Paul, 1957.
Pp. xiv + 336. 32s.

OF the two titles attached to this work the second accurately describes
its main subject ; the other looks like the last-minute inspiration of an
enterprising publisher. About Socrates the man Professor Chroust
has—with one striking exception, to be discussed below—nothing to tell
us, for the good reason that he thinks nothing can be told : ' the only thing
we may confidently assert about the historical Socrates is that we know
practically nothing about him ' (p. 225). His theme is the Socratic myth,
but he attempts no such comprehensive treatment of it as has recently
been undertaken and partially carried through by Magalhães-Vilhena.
Plato, who was surely the mythmaker in chief, is discussed only incidentally ;
Aristotle is dismissed in a sentence, the comedians in a couple of pages ;
and of Xenophon's works only the two ' defences ' of Socrates, *Memorabilia*
1.1–2 and the *Apology*, are analysed in detail—with the object of determining
their sources. The book is in fact a piece of ' Quellenforschung ', of a type
less popular today than it was in the great age of speculative analysis
between 1880 and 1914 ; and it draws heavily on the German specialists
of that period, scholars like Dümmler, Richter, Heinrich Maier, and above
all Karl Joel. That it is the fruit of much industrious research is attested
by the proud array of 1476 notes which occupies its last hundred pages.
But successful Quellenforschung calls for other qualities than diligence in
the compilation of references, and it must be said, regretfully, that Professor
Chroust lacks most of them.

To begin with, he is plainly not at home in the Greek language or with
Greek literary history. He thinks that εὐγένεια means ' eugenics '
(p. 119), that κακῶς λέγειν ἀνθρώπους means ' to speak ill of the people '
(p. 183), that Meletus charged Socrates with being ' a poet ' (n. 232,
quoting *Euthyphro* 3B), that κατερία is a Greek word (p. 113 and *passim*) ;
he perpetually misspells proper names (or allows the printer to do so),
and rejoices in such adjectives as ' Aristophantean ' and ' Eleusian ' ;
he speaks of Plato's *Lysias* (p. 43), and of ' Archelaus of Thessaly ' (p. 165) ;
he classifies Andocides and Isaeus as ' Late Sophists ' (n. 1327) ; he alleges
that ' the diatribe was originally synonymous with the dialogue ' (n. 1355),
and confuses both with the χρεία (p. 210). Strange as such lapses are,
they are perhaps excusable in a professor of law. Nor would it greatly
matter, if he had something important to say, that he says it in very odd
English and with much tedious repetition. But we are entitled to expect
from a lawyer cogent argument and a strict examination of evidence.
We get neither from Professor Chroust. Loose inferences abound :
for example, ' Xenophon's prolonged absence from Athens during *some*
of what may be considered perhaps the most crucial years in the life of
Socrates, made it impossible for him to father *any* first-hand information
about Socrates ' (p. 3, my italics) ; or again, ' Socrates was " all things
to all men ", or, to be more exact, " all philosophies to all philosophers " ;
hence it may safely be concluded that historically speaking he was no
philosopher at all ' (p. 196). As for his assessment of evidence, Chroust's
deep distrust of all the primary witnesses is balanced by a touching faith

in secondary and tertiary ' authorities '. Thus if we wish to reconstruct
the philosophy of Antisthenes, we are reassuringly told (p. 102) that ' we
are permitted to make use of the views of Diogenes of Sinope as they have
been recorded by later authors and historians ' (he means the collections
of anecdotes and bons mots in Diogenes Laertius, Stobaeus, etc.).   We are
even gravely warned (p. 27) that ' the tradition that Socrates worshipped
animals or stones should not be scoffed at ', although this ' tradition ' rests
on the unsupported authority of an anonymous Byzantine who also
believed, among other things, that the Great Plague was occasioned by the
condemnation of Socrates ! Where his authorities disagree—as of course
they often do, especially the tertiary or German ones—Chroust either wavers
helplessly (witness the fearful muddle on pp. 90 ff., 158 and 261 f. over
the much-debated Pindar quotation at *Gorg.* 484B) or else varies his
opinion with the authority he happens to be transcribing. Thus we read
on page 23 that both Xenophon and Plato may have borrowed " the
philosopher's willingness to die " from Antisthenes, but on page 107
that on this point ' Plato's testimony is decidedly not Antisthenian ' ;
the dating of the *Mcno* on page 43 is incompatible with its dating on page
202 ; on page 10 Callicles is identified with Critias, but on page 173 (more
tentatively) with Charicles, while in note 507 he becomes (despite *Gorg.*
520A) ' the sophist Callicles '. The basic weakness of the book is not its
scepticism but its inconsequent mixture of scepticism and credulity.
If its author can tell us little about Socrates, he can tell us a surprising
amount about other people. He knows precisely which Platonic dialogues
preceded the *Apology* in date of composition (p. 43) ; he knows the precise
year in which Polycrates' Κατηγορία was published (p. 44) ; he can date
Memorabilia 1.1–2 (*ibid.*), the *Busiris* of Isocrates (p. 69), and even the lost
Callias and *Alcibiades* of Aeschines (pp. 97, 176). Most of this lore seems
to be taken on trust at second hand ; where reasons are quoted, *e.g.*
Pohlenz's attempt to provide a *terminus ante quem* for Polycrates' work
(p. 73), they are, alas, very far from convincing.

For Chroust the two key figures in the development of the Socratic
myth are Polycrates and Antisthenes. In dealing with the former he is
(chronology apart) on relatively firm ground, thanks to Libanius ; and
while we may doubt if his anti-Socratic pamphlet was quite the ' literary
sensation ' that Chroust imagines, the account of its presumed contents
given in chapter iv will be found useful by American and English readers
(the Polycrates-literature is almost exclusively German). Much more
hazardous are his speculations about Antisthenes. Studies of the in-
fluence of lost authors on extant ones are familiar enough ; but a study
of the influence of one lost author on another has the charm of comparative
novelty, and this is what Chroust offers us (after Joel) in the chapter
entitled ' Antisthenian Elements in the Κατηγορία Σωκράτους of Polycrates '.
His basic principles, here and elsewhere, appear to be that whatever is
said to have been said by Diogenes must have been said first by Antisthenes,
and that if anything remotely similar is said by some one else Antisthenes
must have been his ' source '. For example, when he finds both in
Xenophon's *Apology* and in the pseudo-Platonic *Axiochus* the not very
startling observation ' that old age is something exceedingly burdensome ',
he at once labels this piece of insight ' Antisthenian-Cynic ' : ' for had not
Diogenes of Sinope insisted that the most wretched thing is " an old man
destitute " ? ' (p. 107). The reader may be forgiven if after digesting a
series of such arguments he murmurs ' Antisthenes—Man or Myth ? '
and leans to the second alternative.

But Chroust's most original contribution is reserved for his penultimate chapter, headed ' The Political Aspects of the Socratic Problem '. Here he abandons the simple view that we know nothing at all about the historical Socrates in favour of the proposition that Socrates was ' primarily a *political* rather than a *philosophical* figure ' (p. 191, author's italics)—in fact a sort of *éminence grise* who liked to chat with his friends about the good life but whose real concern was to organise the counter-revolution from behind the scenes. Others of course have emphasised (and with good reason) the political background of Socrates' trial and the political animus of Polycrates ; but has any one suggested before that ' the general Poly-cratean charge that Socrates had planned and (perhaps through the instrumentality of his friends, disciples, followers or partisans) actually brought about the fall of the Athenian democracy in 404, on the whole could very well have been based on historical fact ' (p. 184) ? To maintain this is to maintain that both Xenophon and Plato were outright liars and that Aristophanes totally misrepresented Socrates' real interests and activities. But Chroust does not shrink from these conclusions. ' Natu-rally ', he says, ' all the stories about Socrates' clashes with the Thirty Tyrants can be dismissed as fiction devised by the Socratic apologists ' (p. 174) ; and it was they who turned Socrates ' into a lofty and idealistic philosopher ' (p. 192). As for the *Clouds*, we are offered our choice of several possibilities (pp. 192-4). (*a*) We may assume that in the first edition Socrates talked politics instead of philosophy : ' only in a later version, which might be under the influence of the literary Socratic tradi-tion (!), is he credited with being a " philosopher " '. Or (*b*) we may argue that the play as we have it really proves that Socrates was *not* a philosopher ; for the opinions it attributes to him are not his own, which ' would indicate that Aristophanes could not find a single philosophical statement directly attributable to Socrates '. Or (*c*) we may conjecture that the *phrontisterion* is really ' a witty parody ' of Socrates' political club—perhaps the very same club which under Alcibiades' leadership profaned ' the Eleusian Mysteries '. Need anything more be said ? Like the rest of the book, this chapter is a warning to the inexpert that they had better leave Quellenforschung alone.

E. R. DODDS

Time and Modality. By A. N. PRIOR. Oxford, at the Clarendon Press, 1957. Pp. viii + 148. 25s.

THE author's " conviction that formal logic and general philosophy have more to bring to one another than is sometimes supposed " is admirable. But philosophers differ as to just what they wish to include under " formal logic ". Some, such as Carnap, wish to included the whole of mathematics, as developed, say, on the basis of *Russell's simplified theory of types* or within the *Zermelo* or *von Neumann-Bernays set theory*. Others wish to restrict their logic to, say, a *second-order functional calculus*, within which (on the basis of suitable non-logical axioms) mathematical analysis, the theory of real numbers, may be developed. Still others regard even such calculi as involving too much, and wish to use only a classical *first-order logic*, with perhaps an at most denumerable fundamental domain of entities. Also a *syntax* must be included in the general province of logic, and to this may be added a *denotational semantics* in the manner of Tarski. Some may wish also to add a theory of meaning or intension in the manner of Frege or Church.

To employ an underlying modal or quasi-modal logic for philosophical

purposes is, however, in the eyes of many suspect. Mathematicians have
been interested in modal and many-valued calculi for the sake of the
mathematical structures involved. But philosophers who use modal or
quasi-modal calculi are concerned not merely with mathematical structure.
They must provide also a satisfactory *interpretation* of the calculus or
calculi needed for specific purposes. Of course we must be clear here as to
just what is meant by 'satisfactory'. Roughly speaking, a satisfactory
interpretation may be given for a system only where the meta-language is
sufficiently " clean " and " simple ". Further the interpretation is to be
formulated explicitly in the manner of Tarski's " The Concept of Truth in
Formalized Languages " (in *Logic, Semantics, Metamathematics*) or of
Carnap's *Introduction to Semantics*. It is a controversial point as to whether
any modal or quasi-modal logic can be given a satisfactory interpretation
in this sense. However this may be, the effect of a modal logic may often
be achieved by means of a " clean " meta-language in which a notion of
logical truth is available.

Consider, for example, the definition of 'logical truth' implicit in
Tarski's " On the Concept of Logical Consequence " (also in *Logic, Semantics,
Metamathematics*). To say that a sentence *a* of a given subject-language
L is logically true in *L* is defined in effect by saying that every sequence
of objects is a model of *a*. To say that *a* is satisfiable in *L* is then to say
that the negative of *a* is not logically true in *L*. Between logical truth
and satisfiability relations obtain analogous to those between the modal
notions of *necessity* and *possibility*. But Tarski's meta-language is of the
standard, classical type, the underlying logic consisting merely of truth-
functions, quantifiers, and perhaps identity. There is no need for any
additional " modal " operators or for propositional variables. Most of the
purposes for which a modal logic is designed may be more easily and effi-
ciently served by a clean meta-language of this kind.

It is to be regretted that Prior did not employ meta-languages of the
Tarskian type in this book on tenses. Some of his interesting comments
could then have been given a suitable logical format. As the book stands,
however, it is difficult to sort out the sound from the specious, that which
can be suitably reconstructed from that which is beyond clear analysis.

Prior sketches several modal or quasi-modal theories involving tenses.
He first describes Lukasiewicz's " Ł-modal " system, taking ' *L* ' for
a notion of " necessity " and ' *M* ' for a notion of " possibility ". Then two
tense-operators are introduced, ' *P* ' for ' it has been the case that ' and
' *F* ' for ' it will be the case that '. These operators enable us to form
statements out of statements, so that ' *Pp* ' reads ' it was the case that *p* ',
where ' *p* ' is a statement. A more refined method is introduced involving
variables over natural numbers (which include zero). ' *Fnp* ', *e.g.* now
means ' it was the case *n* days ago that *p* ', if days are taken as the time-
units. And similarly, ' *Fnp* ' reads ' it will be the case *n* days hence
that *p* '. Quantifiers over the numerical variables are admitted
as well as the ordinary truth-functions, so that such complex forms
as ' *NΣnFnNp* ' are significant. (Prior uses the parenthesis-free
notation of Lukasiewicz, ' *N* ' for negation, ' *Σ* ' for ' for some ',
' *Π* ' for ' for all ', and so on.) In terms of ' *F* ' and the other notions
(forgetting ' *P* ' for the moment) Prior outlines an " analogue " of Lewis's
S4, defining

$$\text{' } Lp \text{ as ' } \Pi n Fnp \text{ '}$$

and

$$\text{' } Mp \text{ ' as ' } \Sigma n Fnp \text{ '}.$$

Some axioms and rules are suggested. But presumably an arithmetic is involved also as well as a theory of time. So another operator ' S ', a "time-forming operator on times", is needed "such that the interval Smn may be thought of as the sum of the intervals m and n." But no axioms are given characterizing S. A good deal more is involved here than Prior indicates. Not only do we need presumably a theory of time flow (such as that, *e.g.* used by Woodger), but a metric for time, and an arithmetic to handle the metric. So that a satisfactory formulation of the theory Prior has in mind here would be much more complicated than he indicates.

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Of course if one totally disregards the important modern writings on the semantical concept of truth, one is liable to regard the views which grow out of them as based on prejudice. The point is that we know a good deal about ' true ' or ' true in L ' from modern semantical theory. This theory has not been developed lightly or by amateurs, but is rather the result of a long painstaking tradition. One of the consequences of this theory is that ' true ' is best construed timelessly. For good reasons the scholastics' *consequentiae ut nunc* have fallen by the way. The notions ' was true ' or ' will be true ' are definable, however, in terms of the semantical truth-concept for sentences containing a time indicator. Suppose that ' t ' for the moment is a constant designating a specific time, ' R ' a constant designating a specific relation, and ' x ' a constant designating a specific object. The locution

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as (1). And similarly for sentences of a more complicated form containing a time indicator. So that *consequentiae ut nunc* may easily be handled upon the basis of what is essentially the Quine-Smart view.

If one wishes to complicate semantics now by introducing time-factors in some other way, he is of course free to do it. But let him then face the complexity squarely by developing the full-fledged semantical metalanguage needed, and in careful detail.

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purposes is, however, in the eyes of many suspect. Mathematicians have been interested in modal and many-valued calculi for the sake of the mathematical structures involved. But philosophers who use modal or quasi-modal calculi are concerned not merely with mathematical structure. They must provide also a satisfactory *interpretation* of the calculus or calculi needed for specific purposes. Of course we must be clear here as to just what is meant by 'satisfactory'. Roughly speaking, a satisfactory interpretation may be given for a system only where the meta-language is sufficiently "clean" and "simple". Further the interpretation is to be formulated explicitly in the manner of Tarski's "The Concept of Truth in Formalized Languages" (in *Logic, Semantics, Metamathematics*) or of Carnap's *Introduction to Semantics*. It is a controversial point as to whether any modal or quasi-modal logic can be given a satisfactory interpretation in this sense. However this may be, the effect of a modal logic may often be achieved by means of a "clean" meta-language in which a notion of *logical truth* is available.

Consider, for example, the definition of 'logical truth' implicit in Tarski's "On the Concept of Logical Consequence" (also in *Logic, Semantics, Metamathematics*). To say that a sentence *a* of a given subject-language *L* is logically true in *L* is defined in effect by saying that every sequence of objects is a model of *a*. To say that *a* is satisfiable in *L* is then to say that the negative of *a* is not logically true in *L*. Between logical truth and satisfiability relations obtain analogous to those between the modal notions of *necessity* and *possibility*. But Tarski's meta-language is of the standard, classical type, the underlying logic consisting merely of truth-functions, quantifiers, and perhaps identity. There is no need for any additional "modal" operators or for propositional variables. Most of the purposes for which a modal logic is designed may be more easily and efficiently served by a clean meta-language of this kind.

It is to be regretted that Prior did not employ meta-languages of the Tarskian type in this book on tenses. Some of his interesting comments could then have been given a suitable logical format. As the book stands, however, it is difficult to sort out the sound from the specious, that which can be suitably reconstructed from that which is beyond clear analysis.

Prior sketches several modal or quasi-modal theories involving tenses. He first describes Lukasiewicz's "L-modal" system, taking '*L*' for a notion of "necessity" and '*M*' for a notion of "possibility". Then two tense-operators are introduced, '*P*' for 'it has been the case that' and '*F*' for 'it will be the case that'. These operators enable us to form statements out of statements, so that '*Pp*' reads 'it was the case that *p*', where '*p*' is a statement. A more refined method is introduced involving variables over natural numbers (which include zero). '*Pnp*', *e.g.* now means 'it was the case *n* days ago that *p*', if days are taken as the time-units. And similarly, '*Fnp*' reads 'it will be the case *n* days hence that *p*'. Quantifiers over the numerical variables are admitted as well as the ordinary truth-functions, so that such complex forms as '*NΣnFnNp*' are significant. (Prior uses the parenthesis-free notation of Lukasiewicz, '*N*' for negation, '*Σ*' for 'for some', '*Π*' for 'for all', and so on.) In terms of '*F*' and the other notions (forgetting '*P*' for the moment) Prior outlines an "analogue" of Lewis's *S4*, defining

'*Lp* as '*ΠnFnp*'

and

'*Mp*' as '*ΣnFnp*'.

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18

spatial factor also. He could equally well attempt to characterize ' true
here ' or ' true there ', or even ' true at such and such a temperature ',
and so on, in quasi-modal terms. Consider, *e.g.* the following sentence
from Russell's *Portraits from Memory* (p. 199).   " There are a great many
people who have the material conditions of happiness, *i.e.* health and a
sufficient income, and who, nevertheless, are profoundly unhappy. This
is especially true in America."   Russell could have said here equally well
' true *of* America '.   But in terms of ' true ' or ' true of ' we can surely
define ' true *in* ' in the sense in which Russell is using it.   But one would
scarcely wish to write a whole book on space and modality to explicate this
sense.   Also Prior's ' *Fnp* ', *e.g.* can no doubt be interpreted as meaning
' *p* is true at *n* degrees Centigrade '.   But the resulting theory would be
quite trivial and uninteresting from the point of view of the theory of
temperature.

The fact is that time and space and temperature are not the affair
of a semantics, which need not contain terms from physics. Expressions
for time, space, etc., are best incorporated in the physical object-language.
Of course, if the object-language contains expressions for time then the
semantical meta-language does also.   But Prior seems to want a " logic "
of tenses even where the sentences of the object-language contain no time
indicators.   The Quine-Smart view is not based upon prejudice but rather
upon a sound and well-developed methodology.   One is free to reject this
methodology just as one is free to reject, say, quantum physics.   But it
behooves one then to put forward a better alternative.

Prior worries a good deal about the " sempiternity hypothesis ", roughly
" that whatever exists at any time exists at all times ", which seems to
underlie his theories.   Therefore a revision is in order, culminating in his
" System Q ".   But here we are to imagine that some of our statements
are perhaps " true today and unstable yesterday ", with no clear explanation
given as to what ' unstable ' means.   Let ' *Mp* ' mean now ' *p* at some time '
and ' *Lp* ' ' *p* at all times '.   Within Q, the operators *L* and *NMN* are not
identified and as a result Q apparently contains some rather novel features
as a modal logic.   Objections to regarding it as a suitable tool for analysing
tenses are similar to those above.

Prior also considers a system which results from Q by grafting on " the
Russellian name-and-predicate calculus ", and another by using something
like Léśniewski's *ontology*.   Prior himself puts forward several objections
to these, some of which he thinks can be overcome.   Finally, there is a
discussion of such locutions as ' it is believed that ', ' it is desired that ',
etc., and their connexion with indeterminism.

The philosopher of science in search of an analysis of time or tense as
employed, say, in physics or psychology or sociology, will gain little from
Prior's book.   He will demand a more subtle and exact analysis of the
behaviour of suitable expressions for time within scientific language.

On page 55, there is a rather extraordinary but erroneous statement
to the effect that the " typical Russellian apparatus of logical proper names,
predicates, and descriptions " . . . " is at present a little out of fashion ".
On page 64, Prior says that Russell " does not define *ıxɸx*, but only gives
equivalents for the main contexts in which it occurs."   Surely this state-
ment rests on a misunderstanding of *14.01 of *Principia Mathematica*,
in which *all* contexts in which ' (*ıx*) (*ɸx*) ' can significantly occur are
suitably defined.   Throughout Prior speaks of " variable operators "
where operator variables are meant.

The following errata may be noted. Page 19, line 4, for ' even ' read
' event '; page 27, the second exhibited formula should be

$$C\varSigma t \varSigma x \phi xt \varSigma x \varSigma t \phi xt.$$

The author wishes to point out that, on page 23, the matrix said to be
characteristic of $S4$ is characteristic rather of a " hitherto undiscussed
system between $S4$ and $S5$ ", as was noted by Mr. John Lemmon. Also
in the large diagram on page 124, ML ought to be connected with MLM.
On page 132, the capital ' P ' should be a small one.

<div align="right">R. M. MARTIN</div>

Minnesota Studies in the Philosophy of Science. Vol. ii : *Concepts, Theories,
and the Mind-Body Problem.* Edited by H. FEIGL, M. SCRIVEN, and
G. MAXWELL. University of Minnesota Press, Minneapolis. Pp.
xv + 539.

THE most interesting and critical paper in this collection is, I think,
K. Zener's ' The Significance of the Experience of the Individual for the
Science of Psychology '. It encourages the trend of research into con-
scious experience in spite of it being difficult to design repeatable experi-
ments in this field and in spite of the highly interpretative character of
the resulting observational reports. It is also the only paper which is
too brief. Interesting examples are suppressed. Its critical thesis is that
behaviourism, rightly searching for intersubjectively acceptable evidence,
has aped the behaviuoral aspect of experimental physics but misunderstood
its function in physics. In support of Zener's ideas I should like to add
that his view of the experimenter's work as bold and creative is in no way
alien to experimental physics even though some of its imitators were too
impressed by its pedestrian aspect to notice its imaginative and exciting
aspect. Indeed, by the behaviourist's standard all scientific experiments
are equally significant, while few experiments equal those of Oersted,
Hertz, Michelson, and Rutherford.

H. Feigl's ' The " Mental " and the " Physical " ' ' lacks a compact and
systematic presentation. Its first two sections, however, are brief, clear,
and interesting. Feigl's theory is that the mental process is a high-level
physiological process. Transcending logical empiricism he explicitly
recognizes his thesis as synthetic-yet-speculative. He also ends with a
list of open problems. It is regrettable that he shoves off the main
objection to his refined epiphenomenalism, namely, the dualism of facts
and decisions, to another field, thus declaring it irrelevant rather than still
unanswered (p. 418). He is slightly unfair once, when he criticizes
dualistic interactionism on the ground that methodological monism (which
requires us to try to formulate physicalist hypotheses to explain human
behaviour) has been successful. For, methodological dualism has also been
successful, and Feigl himself admits, *en passant* (p. 463), that methodologi-
cal dualism is at present unavoidable. This admission invalidates the
criticism on pp. 383-6. (Zener's paper forcefully advocates methodological
dualism.)

The first paper in this volume, by P. Oppenheim and H. Putnam, on the
unity of science, is a retreat to Comte's famous theory of the hierarchy of
the sciences. Unfortunately it completely ignores Comte.

The second paper, by C. G. Hempel, poses the theory that hypotheses
are ' economic systematizations ' (p. 87) with no mention of Mach, Duhem,
or Poincaré. It seems to me vague and unconvincing. He poses one

problem, discusses another problem, and offers a solution to yet another problem, without discussing the relations between the three. (The problems are, why do we assume occult entities to explain known ones, how do we know the meaning of terms which stand for these occult entities, and why do we use explanations at all.) His chief problem is whether or not explanations are redundant. He calls his paper ' The Theoretician's Dilemma ', but I would rather prefer the title ' The Half-hearted Theoretician's Dilemma ' ; for the theoretician wants mainly to find satisfactory explanations, and this aim leaves no room for Hempel's problem which, however, may exist for the instrumentalist whose aim is not explanation but prediction (for technological purposes). Hempel takes for granted the instrumentalist philosophy and he does not notice that the theoretician's prediction is not merely for the aid of technology but mainly and chiefly for the sake of testing his explanation.[1]

Next M. Scriven asserts the conditional, *if* theories are redundant (for the purpose of describing the world), *then* theoretical terms are redundant (*i.e.* definable, at least in a wide sense of ' definition '). He devotes his paper to the contention that the consequence of this conditional is false, in order to conclude that its antecedent is false too.

A. Pap and W. Sellars add to the vast literature on dispositionals without telling the reader in simple words what is worrying them. In my view dispositionals can worry only those who think that some observation-reports are *absolutely* certain. Against this Popper argued (1935) that every universal word (' glass ') entails a dispositional (' breakable '), so that every statement employing it (' here is a glass of water ') is further testable and therefore can never be certain. The attempt to discover statements of brute facts has swollen the literature on dispositions ; how much the present papers contribute, either to the problem or to its solution, I cannot judge.

H. G. Alexander argues that the view of hypotheses as licences to infer one particular from another (Mill, Schlick) is misleading since it obscures the fact that our hypotheses may be false .

H. P. Strawson's discussion ' On Persons ' ends with two points which he seems to think are identical. The one (p. 352) advocates ' the logical primitiveness of the concept of a person ', the other (p. 353) defends ' the conceptual scheme we employ ' in ordinary everyday discourse. I accept the second point which is, in intention at least, a refinement of the common-

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[1] This is Feigl's criticism if his ' factual reference ' means what ordinarily ' truth or falsity ' means. See page 427, passage commencing ' I remain unimpressed with Craig's theorem ' (which concerns a method of elimination of theoretical concepts). Feigl seems to suggest that historically Craig's theorem is the cause of the ' theoretician's dilemma ' yet that intellectually it is a false alarm. If so, it should be added, in order to avoid misunderstanding, that Craig himself tries (*Phil. Rev.*, 65 : 38–55, 1956) to show that any elimination of a concept must be pointless, thus arguing that there is no ' theoretician's dilemma '. It is odd that Hempel does not try to answer Craig's arguments, or even state that in Craig's view there is no dilemma. He suggests, by hints too subtle to discuss here, that Craig is for eliminations, and that his theorem is of mere logical, but of no philosophical, significance. Craig himself does not over-estimate his theorem (which is but a corollary of Kleene's theorem). And he uses it very competently as an argument against the philosophical view imputed to him by implication. And he uses himself, among other ideas, most of the criticisms which Hempel and Feigl appear to level against him. The interested reader should not rely on Hempel's report but read Craig's own presentation.

sense view on the matter, yet cannot see what 'logical primitiveness' has to do with commonsense or with views. Which concept should be taken as primitive, or undefined, is of interest only in logic, where often such a question is shown to be partly arbitrarily decidable. If we use the term 'logical primitive' loosely, we should notice that commonsense views persons (especially those with 'dual personality') as highly complex.

P. E. Meehl's is a disappointing paper apropos clinical psychology with the splendid title 'When shall We use our Heads instead of the Formula ?'.

The Appendix contains a correspondence between W. Sellars and M. Chisolm on the relation between the mind-body problem and intentionality in logic. I could not see its point.

The worst thing about the present volume is its editing ; much could and a great deal should have been done by the editors to improve the volume. If the contemporary tradition of publishing endless collections goes much further, ideas will be drowned in a frantic flood of words. 'Above all, my son, beware ', said the Preacher, ' of the making of many books without end, and of much tiresome prattle. . . . '

<div align="right">J. AGASSI</div>

The Province of Jurisprudence Determined, Etc. By JOHN AUSTIN, with an introduction by Professor H. L. A. Hart. Weidenfeld and Nicolson. Pp. xxi + 396. 12s. 6d.

THIS is another volume in the useful Library of Ideas. It contains the complete text of The Province of Jurisprudence including a careful and fairly extensive defence of Utilitarianism, excluded from earlier editions, on the lines that Utility is a test of rules of conduct and not applicable to particular actions except in 'anomalous' cases. An essay, The Uses of the Study of Jurisprudence, in which Austin outlines his conception of the subject is also reprinted. There is an excellent bibliography.

Professor Hart summarizes Austin's career and influence and comments briefly on some central ideas in his 'half-illuminating, half obscuring' account of law. The root fault he locates in Austin's choice of ' command ' and ' habit ' as key terms rather than ' rule ' and ' ought '. A legal system is not a system of commands issuing from a sovereign but of rules identifiable in a manner determined by a generally accepted constitutional rule. This (substantial) revision would clear away the more arbitrary features of the distinction between laws properly and improperly so called and reveal the logically necessary role of constitutional rules in any legal system. Hart questions the wisdom of pressing the analytic—synthetic distinction in discussing the connexion between law and sanctions and law and its varied human functions.

It is good that Austin's tough, painstaking lectures should be easily available when English philosophers are beginning to interest themselves again in the problems he discussed.

<div align="right">ROBERT KIRKHAM</div>

The Philosophy of Kant and Our Modern World. Edited by CHARLES W. HENDEL. The Liberal Arts Press, New York, 1957. Pp. 132. $2.75.

THESE four lectures were delivered to a non-specialist audience at Yale University to commemmorate the 150th anniversary of Kant's death. If there were a Kantian religion, they would rank high as sermons in it.

They are very general, quite readable, neither particularly misleading nor inaccurate, and attractively concise. The book itself is well produced. The topics chosen derive almost exclusively from the moral, political and aesthetic sides of Kant's philosophy. The first two lectures discuss Kant's general metaphysical programme (' The question of Man ') and his affinities with existentialism (' The philosophy of existence '). The third, by Professor Wellek, on ' Aesthetics and criticism ' is a general and informative survey of Kant's views in their relation to those of contemporary and succeeding writers on the subject. The fourth, by the editor, on ' Freedom, democracy and peace ' outlines some of Kant's political beliefs. It is not hard to imagine that the lectures were received with enthusiasm, as the editor reports ; though they do not sparkle, they are far from dull.

G. H. BIRD

Words and Images. By E. L. MASCALL.   Longmans.   Pp. xi + 132. 12s. 6d.

THE author of this book describes his purpose as " . . . providing a rational justification for the activities of thinking and talking about God ".

In his first chapter his main target is A. J. Ayer. Of the Verification Principle he asks : since Ayer altered the Principle in order to be able to say that general statements and statements about the past are meaningful, why can we not alter it again and make it cover theological propositions ? But Ayer thought that the Principle had to be altered in order to cover empirical statements in general, that is, he thought that in its strong form it could cover only a very small number of empirical statements. The Principle was wrongly expressed in the first place. " Mitigating " it in order to make it cover the very large range of propositions called empirical is different from mitigating it to make it cover a type of discourse which according to Dr. Mascall is made up largely of analogues, has a subject-matter not only unique, but essentially mysterious, and which, it seems, is delineated by its subject-matter.

The old complaint about the status of the Verification Principle itself, is also brought up. Although it may or may not be true that meaningful statements group themselves about two poles called " Verifiable in Experience " and " True by Definition ", if someone believes it is not true it is up to him to modify the account in some way : to show for instance, that propositions scatter between the poles, or that the poles are in the wrong place, or that there are more than two poles ; if this is not done, it is not only the status of the Verification Principle that remains in doubt.

Chapter 2, and most of Chapter 4 are devoted to a discussion of perception. This is because Dr. Mascall believes that the only way to escape from Phenomenalism is to admit the existence of a non-reasoning, sympathetic, passive, contemplative faculty of the intellect ; a faculty, that is, which allows us to grasp real things, as against sensations, and this faculty enables us to know that God exists. But this kind of assertion is not a way of " . . . providing a justification for . . . talking about God ", or about physical objects, or about anything.

In Chapter 5, Dr. Mascall says that Theology " has its own way of talking ", just like Physics, Poetry, and Philosophy. But in what way is it " Just like ? " The point of talking about " Different languages," and of remarks like " Every expression has its own logic " is either to begin to indicate a source of the confusions arising from the fact that people can get very muddled about the implications of some of the things they say,

or else to summarise in a slogan something about the nature of such confusions. Dr. Mascall, however, appears to believe that by saying " Such-and-such has its own way of talking", one gives a guarantee against serious confusion. There is no reason to believe in such a guarantee until we know what is meant by " A way of talking." It is no good to be told that theological discourse often consists of metaphors and analogies, or that its subject-matter is unique.

Chapter 3 is about Braithwaite's account of religious belief. There is also a short section on the problem of evil.

<div align="right">JENNY TEICHMANN</div>

Existentialism and Religious Belief. By DAVID E. ROBERTS. Edited by ROGER HAZELTON. Oxford University Press, New York, 1957. Pp. 344. 30s.

THIS book, mainly completed before David Edwards's death in 1955 and rounded off (and partly rewritten) by the editor, surveys existentialist writings with a view to showing their relevance to Christian theology. He gives an exposition of Pascal, Kierkegaard (two chapters), Heidegger, Sartre (rather more briefly), Jaspers and Marcel. The first of these authors is included as being a precursor of modern existentialism, on the grounds of his awareness of the ' incongruity between man and nature, the sharp contrast between subjective . . . and objective knowledge ' (p. 58). On the whole his exposition is clear, in relation to the mysteriousness of much of the subject-matter, and especially good in respect of Heidegger, whose dark sayings become more intelligible and impressive when viewed as reflecting ' something like the mystic's awareness of the temporal world ' (p. 189, with special reference to the notion of ' being thrown into existence '). Though the body of each chapter is expository, he ends each survey with a brief critique. Some of these are interesting, though they also sometimes do not appear clear : as, *e.g.* when speaking of Sartre he remarks ' One can be an atheist without adopting Sartre's philosophy ' (p. 224) and yet later goes on to say ' If I am deceived when I choose to believe in God, then I lose nothing of any real value, since if this kind of atheism (*sc.* Sartre's) is true, all values disintegrate completely. Thus the choice is put before one in the sharpest possible form.' What kind of sharpness, this ? In conclusion he lists the features of religious importance in existentialism. (i) It warns against an idolatrous attitude to science ; (ii) it offers a drastically realistic acquaintance with the stuff of human existence ; (iii) it illuminates the vexed relationship between faith and reason, for the new philosophy may ontologise from inside the vivid personal language of drama and poetry ; (iv) it provides an analysis of unfaith and hence is indispensable to apologists ; (v) it draws attention to the problem of human freedom. Of these, perhaps (iv) is the most suggestive, for even though the spirit of existentialism seems irrationalist, it contains much that is, while not being natural theology in the old sense, a sort of religious reasoning.

<div align="right">R. N. SMART</div>

The Language of Value. Edited by RAY LEPLEY. Columbia University Press (London : Oxford University Press), 1957. Pp. vi + 428. Price in U.K. 52s.

THIS book contains a series of twelve essays by various authors on widely different aspects of the language of value and value theory, together with mutual criticisms and rejoinders. It is a sequel to an earlier volume of

similar pattern, *Value: A Co-operative Enquiry* (ed. Lepley, New York, 1949), which in turn arose out of an attempt by John Dewey in 1944 to re-state the major questions of value theory. Out of such earlier axiological studies came a recognition of the need for a discussion of the semantics of value judgments, and of the desirability of reducing the tangle of te..-minology. Both books are inspired by the conviction that ethics can be established as an empirical science firmly based upon an agreed conceptual framework. Whether or not this conviction constitutes an illusion, the symposiasts are under no illusions as to the difficulty or remoteness of their goal. Within a general empiricist relativism, their views in fact differ quite widely. The clearest divide is between the adherents of a conative (*e.g.* instrumentalist) theory on the one hand and those of an affective (*e.g.* hedonic) theory on the other, with a corresponding disagree-ment about the validity of a purely behavioural or behaviouristic approach (there is some heated confusion about how these terms are related). In general, the spirit of Dewey broods over many a page on which his name is not mentioned, and there are traces in both volumes of his somewhat unphilosophical phobia of a doctrine called " absolutistic ethics ", which is more often inveighed against than examined. This occupational neurosis of the pragmatist, which seems to be based partly on a mistrust of authority, is perhaps partly to blame for the fact that in discussing value in general rather than simply ethics (in itself a laudable programme) nearly every symposiast fails entirely to leave room for the distinction between prudence and morality ; the reaction against deontological ethics goes altogether too far.

But another reason for this failing is a confusion more central to the subject of the present book—a confusion over the relation of semiotic to value theory ; and I shall concentrate on this point. The general problem of the relation of semiotic to axiology is discussed in a fair-minded article by E. S. Robinson, which brings us to the heart of the confusion. He criticises Charles Morris's *Signs, Language and Behaviour*, which he believes to demonstrate the impossibility of producing, as Morris earlier aimed at doing, a theory of signs independent of both philosophy and science in its assumptions and its basic vocabulary. On this point he and Morris seem to reach a measure of agreement ; semiotic is no longer conceived as an independent organon but simply as one aspect of each scientific or philosophical field. But Robinson himself, in the constructive part of his paper, gets involved in a similar difficulty. He suggests a classification of value-terms under such headings as ' valuational ' and ' motivational ' (these words being ostensively defined by the lists), which he admits is open to the criticism that expressions in one category may, when one comes to value-theory, turn out to be reductively definable in terms of expressions in another ; in other words, even a preliminary semiotic scheme implies a theory of value. He seems to me to be right in saying that " the relations between sign theory and value theory may not be in a tangle, but . . . they are still obscure ". For some of his collaborators this is an under-statement.

The two essays which seem to me to avoid best the confusion are those by Garnett and Brandt, in which, as in the best recent English contri-butions, conclusions about the logical status of ethical expressions emerge from a careful conceptual analysis in the area concerned. Campbell Garnett, on " A non-normative definition of good ", has contributed a well-argued and convincing paper which provokes from Fingarette one of the most fruitful sets of comments. R. B. Brandt poses " Some puzzles

for attitude theories of value "; he questions, from a "cognitivist" point of view, the ability of attitude theories to deal with (1) the fact that value statements and attitudes do not always coincide; (2) criteria of ethical relevance; and (3) the effectiveness of certain types of ethical reason. The comments, appropriately, are by C. L. Stevenson, and together with Brandt's response provide an excellent illustration of the interpretative scope of the two theories.

Perhaps the most important and stimulating essay in the book is R. S. Hartman's "Value Propositions", which develops with much subtlety a formal logic of axiological statements; briefly, his thesis is based on a distinction between the definition and the empirical exposition of a concept, and he holds that a thing is good in proportion as it possesses the qualities of its exposition. But I am not clear that this paper avoids errors about the relation of axiology to semiotic, because it seems to me that the choice of what to include in the exposition of a concept may well presuppose value judgments, and is not a purely logical affair. Hartman has developed this question of the discovery of the exposition in a paper which I have not seen (reference on p. 337); but his remark that "the possibilities of agreement [about the exposition of a concept] can actually be mathematically computed " leaves me apprehensive.

Morris in his own paper attempts an empirical discovery of the meaning of appraisal terms by examining the responses of a group of students to a number of paintings, and comparing their "preference ratings " with their "appraisal ratings ". The subjects in giving their P-ratings were " told explicitly not to judge the picture as a work of art ", a procedure which not only, as McGreal points out, vitiates the experiment on the assumption that the general plan is sound, but also illustrates, to my mind, the fact that the interpretation of such empirical data presupposes, rather than produces, a logical analysis of the concepts involved. The error is not, now, that the function of axiological terms is deduced from a somewhat a priori semiotic; actual axiological situations are now being studied in the most literal sense. The error this time is to substitute for conceptual analysis (which takes as its point of departure human experience at its richest, most reflective and most consistent) a purely contextual analysis of the function of axiological terms, whose outcome is in any case prejudiced by the conceptual framework covertly introduced by the investigator. The presence of a similar mistake is noticed in Herbert Fingarette's paper by Garnett (cf. p. 311), who points out that the use of an ethical term depends on its meaning. The study of semantic function in place of definition can only lead to the obliteration of the sort of vital distinction (e.g. that between prudence and morality) that Garnett's own essay makes so clearly.

S. C. Pepper offers " an empirically responsible procedure for evaluating values " (p. 285). This seems to mean that we test concrete values by referring them to an empirical definition of values in that particular field, and Pepper lays down the conditions of adequacy which such a definition must meet. But, as is clear from his illustrative definitions, e.g. of value as " the achievement of a purpose " (p. 88) or as " the integration of the dispositions composing a personality structure " (p. 91), Pepper confuses the descriptive definition of that in which value resides with the definition of value. A case could be made out for offering these as definitions of value, but Pepper is clearly not doing this; his quest belongs to psychology or sociology, and Lafleur rightly says (p. 283) that "the ethical question cannot be answered in this way ". E. M. Adam's general position is not

dissimilar. In his criticism of the view that value statements are non-factual, he reduces all ethical appraisals to means-ends statements and gives no clue to why we should adopt one end, satisfy one " relevant interest ", rather than another.

Ian McGreal, in a dialogue of elegant argument and disastrous wit, attempts a reconciliation between " essentialist " and " semanticist " theories of meaning. Where in the past philosophers have vainly searched for the " real meaning " of a word, they may now realise that what they were after was simply that particular mode of signification in which they were personally interested. But this proposal, and the implied criticism of the search for " real meanings " which most of us nowadays would echo, are only relevant to philosophers who *were* primarily concerned with the discovery of one privileged mode of signification for a word. One supposes that Plato would qualify as the doyen of essentialists, and no doubt Plato was over-zealous for univocity. But surely he, and most of the traditional moral philosophers after him, have been concerned chiefly with a type of analysis to which verbal definition was merely instrumental ; and if so, McGreal's proposal misses the point. One senses again the confusion between conceptual analysis and the description of semantic function.

The papers by Willis Moore and Harold Lee suffer from their having tried to shoehorn an entire ethical theory into an article. Moore's paper, perhaps the least satisfactory of all, offers a naive social approval theory of ethics which reduces all axiological judgments to likings, with the rider that an axiological situation " in an area of strong social pressure " is known as a moral situation. A fair sample of the argument occurs on page 20, where Moore says that from the fact that users of ' ought ' or hortative expressions are " expected to complete the exhortation by mentioning some supporting reason, . . . *it follows* that all hortative sentences, moral or not, are a special case of means-end sentences ". My italics.

In his paper on " Intrinsic Value ", Lee starts from the point that the " noun sense " of the word ' value ' is " semantically prior " to the " verb sense ". Semantical priority depends on the " most general sense " of the word (p. 180) : "when I value an object, I am finding value in it or ascribing value to it " (p. 181). But, after all, this turns out to be only a linguistic stipulation on Lee's part (p. 181) ; he is not talking ontology (p. 331). " The study of semantics by itself solves only semantic problems " (p. 183). Yet his semantic recommendations are compatible with some theories of value and not others (pp. 182–183), and he himself puts forward a theory which he calls ' evaluative hedonism ' which " offers a much simpler semantics for axiology than does definitive hedonism " (which *defines* value in terms of pleasure) ; and on p. 195 we read that the choice " is more than terminological. It is semantic in the way that semantics and theory are inseparable ". I find this confusing.

It remains to mention a somewhat complex review of the state of value theory and value terminology by the editor, Ray Lepley, and the final overall comments by Lepley and Hartman, the former from " an inclusive experimental standpoint ", the latter concerned with distinguishing the language levels employed by each contributor. Hartman scores over Lepley on both appearances by selecting the more restricted topics ; throughout the book the more specialised articles are the most rewarding even when one does not agree with them.

The book would be less irritating to read if the comments and responses had appeared after the several essays to which they apply. The system

inherited from the earlier volume, in which the comments each dealt with more than one essay, is unnecessary and frustrating in this. The presentation of the book is otherwise pleasing, which is more than one can say of the price.

N. J. Brown

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XII.—NOTES

JOINT SESSION

St. Andrews University, 10–12 July, 1959

PROGRAMME

Friday 10 July   8 p.m. Chairman : Professor K. R. Popper.
Inaugural Address by Professor J. N. Wright.
Mind and The Concept of Mind.

Saturday 11 July   10 a.m.   (a) Symposium : Is there only one correct system of Modal Logic ?

Chairman : Mr. W. C. Kneale.
Mr. E. J. Lemmon, Mr. G. P. Henderson.

(b) Symposium : Pleasure and Belief.
Chairman : Professor J. O. Urmson.
Mr. B. A. O. Williams, Mr. E. Bedford.

8 p.m. Symposium: Phenomenology and Linguistic Analysis.
Chairman : Professor Gilbert Ryle.
Mr. C. E. Taylor, Professor A. J. Ayer.

Sunday 12 July   10 a.m.   (a) Symposium : Determinables and the Notion of Likeness.

Chairman : Professor R. B. Braithwaite.
Professor S. Korner, Mr. John Searle.

(b) Symposium : Rules of Morality.
Chairman : Professor H. L. A. Hart.
Mr. Neil Cooper, Mr. R. Edgley.

8 p.m. Symposium : Paradox in Religion.
Chairman : Professor D. M. MacKinnon.
Professor Ian Ramsey, Mr. N. Smart.

Colloquium

on

CONTEMPORARY BRITISH PHILOSOPHY

ITS SCOPE AND RELATIONS

A Colloquium, which will have the support of members of the Board of Philosophical Studies and of other teachers in the University of London, will be held in London from 8 to 18 September, 1959, under the auspices of the British Council. Professor H. B. Acton, M.A., D.Phil., Professor of Philosophy at Bedford College in the University of London, has agreed to act as Director of Studies.

The Colloquium is designed for teachers of philosophy in universities and schools, research students of philosophy and others who are professionally interested in the subject, and it is hoped that membership will be largely drawn from European and other overseas countries.

In the course of previous colloquia, held at Cambridge in 1953 and at Oxford in 1955, an account was given of the analytic and linguistic methods characteristic of British philosophy during the last few decades. At the London colloquium an attempt will be made to indicate the wide variety of views and

methods which are developing in British philosophy today, and among the
subjects to be treated will be the relations of philosophy to other branches of
knowledge as well as problems of epistemology, ethics and aesthetics.

Further information may be obtained from the overseas offices of the British
Council, or from the Director, Courses Department, The British Council, 65
Davies Street, London, W.1.

ERRATUM

Owing to a proof-reading lapse on the part of the author of " Experience and
Reality " (MIND, July 1958) for which he apologises to the Editor and readers
of this journal, the second last line on p. 391, which now reads " subsuming my
objective data under the appropriate objective form ", should read " subsuming
my *subjective* data under the appropriate objective form."

BACK NUMBERS OF "MIND"

Mr. L. G. Whitehead of 7 Brodie Street, Upper Riccarton, Christchurch,
N.W.3, New Zealand has a run of unbound MINDS (October 1908 to October
1958) for disposal.   The cost would be £1 per volume, plus packing and carriage
expenses.

Mr. C. Johnson of 67 Princes Square, London, W.2, has for disposal MIND
1952–1956, plus MIND of April 1957 and of January 1958.

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CONTENTS FOR APRIL 1959

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CONTENTS FOR DECEMBER 1958

CONTENTS

CONTENTS